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Winter 1978–79

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Edwin A. Weinstein, James William Anderson, Arthur S. Link — Woodrow Wilson's Political Personality: A Reappraisal

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Gerald M. Pomper — The Impact of *The American Voter* on Political Science

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Political Science Quarterly

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From the Managing Editor:

In this issue . . .

BERNARD GIFFORD traces the economic decline of New York City vis-à-vis the rest of the nation to the economic and social policies of the New Deal, the quintessential expression of New York's liberal political ideology. He posits that the recent economic difficulties of New York, coupled with increasing attention to computations on revenue flows between New York and Washington, may lead to a "new" New York political ideology.

EDWIN A. WEINSTEIN, JAMES W. ANDERSON, and ARTHUR S. LINK review the literature on Woodrow Wilson's personality, concentrating on the book *Woodrow Wilson and Colonel House* by Alexander and Juliette George. The authors contend that the Georges' book is based on incorrect assumptions about the relationship between Wilson and his father and offer an alternative view of the dynamics of Wilson's political personality.

BERNARD BARBER examines the nature of the powerful professions in American society, the extent to which they fulfill their claims to effective self-regulation and public responsibility, the criticisms to which these professions are subjected, and the reforms suggested for them.

GERALD M. POMPER discusses the impact of the classic study of electoral behavior, *The American Voter*. He analyzes the sources of its influence, identifies disputed propositions from this seminal work, and suggests directions for future work in this field.

MICHAEL ROSKIN analyzes Spain's first free elections in forty-one years. He characterizes them as "shakedown" elections in which too many parties and personalities competed for a basically moderate electorate and learned some lessons in political science.

GEORGE H. QUESTER argues that an examination of American and foreign public opinion for the years after 1945 can shed new light on the issue of "who started the cold war?" A crucial finding is that such opinion saw the cold war as emerging only slowly and gradually, rather than erupting directly at the end of World War II.

CANDICE J. NELSON shows a strong tendency among voters in congressional elections to deviate from their party affiliation when one of the candidates is an incumbent from the opposite party.

DEMETRIOS CARALEY

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New York City and Cosmopolitan Liberalism

BERNARD R. GIFFORD

In a tart-tongued 1968 essay, "Who knows New York?—Notes on a Mixed-Up City," Irving Kristol and Paul Weaver, after pointing out that "New York is unique among American cities for the vast ignorance and odd misimpressions which its own inhabitants—including the most educated and the most powerful—have of it," went on to make a prediction about the future of the city:

> Everyone knows . . . that New York City is in the process of rapidly acquiring a nonwhite majority, and the city will have to adapt itself—painfully, but inevitably—to this radically new situation. But what everyone knows is, in this case as in so many others, not so. It is extremely unlikely that this nonwhite majority could arrive before 1990; and it is very unlikely that it will arrive at all.[1]

Of course, Kristol and Weaver were wrong. According to the 1974 population survey, nonwhites and Hispanics currently constitute 47 percent of the city's total population—3.6 out of 7.5 million.[2] Even more significant, 60 percent of the age group comprised of individuals 14 and under is nonwhite and Hispanic. Based on these figures, it seems likely that the 1980 census will reveal a nonwhite majority in New York City. Kristol and Weaver also predicted that:

[1] Irving Kristol and Paul Weaver, "Who Knows New York—Notes on a Mixed-Up City," *The Public Interest* 16 (Summer 1969): 41.

[2] Arley Bondarin, *New York City's Population—1974: Socioeconomic Characteristics from the Current Population Survey* (New York: New School for Social Research, 1976).

BERNARD R. GIFFORD is resident scholar and program director, New York City Studies, at the Russell Sage Foundation. He previously served as president of the New York City-Rand Institute and Deputy Chancellor, New York City public schools. Dr. Gifford has written numerous articles and monographs on policy analysis and public management, resource allocation policies in municipal and state government, and school finance reform.

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> As active participants in city politics, liberals have become confused and divided; their ideological instincts have become uncertain; and their influence has dissipated. . . . New York has seemingly passed beyond liberalism to—well, what New York is coming to is impossible to foretell, but how it is getting there is only too clear.[3]

By taking a tentative stab into uncertain ground, Kristol and Weaver struck gold. New York City, having reached a state of *de facto* bankruptcy almost three years ago and now finding itself in the midst of a fierce struggle to ward off *de jure* bankruptcy, has been forced to jettison many of its cherished liberal traditions. A city living on the precipice of national disgrace—and bankruptcy means just that—is hardly in the position or the mood to equate expenditures of funds it does not have with the public good.

The purpose of this retrospective glance at predictions made almost a decade ago by two keen observers of the New York scene is not to pass judgment. Rather, the point is to illustrate that *specific* predictions about the future of New York City are risky propositions; vague, *general* predictions are more likely to hold up over time. Unfortunately, however, the latter often fail to provide insight into why a particular event—for example, a change in the way the polity responds to a problem—takes place.

Despite the perils involved in making predictions about the future of New York City, the purpose of this essay is to do just that. In particular, we will address ourselves to the impact of the city's economic decline and *de facto* default on intergovernmental fiscal relations. We will place our findings, in turn, in the context of New York's long history as a "social welfare city." Our approach and our prognostications will be modest; wherever possible, we will buttress our predictions with factual information, though we freely admit that many of the most important changes in the life of the city are not likely to be found in computer printouts.

Above all, we are interested in exploring the narrow—and some would contend, nasty—array of policy alternatives for dealing with decline that public officials will be considering for at least the next decade. But let us begin. As so we shall—with a prediction.

Decline Is an Old Story

The economy of New York City will continue to experience, for at least the next decade, a rate of economic growth far lower than that of most of the rest of of the nation. Is this a revelatory observation? Hardly. On the heels of a mayoral campaign in which the major issue separating the candidates was the degree of their apparent willingness, in the face of economic decline, to impose marrow-deep cuts in basic municipal services, New York's bleak economic prospects are certainly nothing new. What many may not realize, however, is the fact that New York City's economy, as measured by the rate of increase

[3] Kristol and Weaver, "Who Knows New York," p. 58.

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TABLE 1

Changes in Per Capita Personal Income for the United States and New York City: 1940–1975

	Per Capita Personal Income					Percent Change In Real Per Capita Income		
	Current Dollars		Constant Dollars**					
	United States	New York City	United States	New York City	Period	United States	New York City	Ratio
1940	$ 592	$1,027	$1,410	$2,422				
					1940–1950	47.2%	23.9%	0.51
1950	1,496	2,136	2,075	3,000				
					1950–1960	20.7	12.3	0.59
1960	2,222	2,941*	2,505	3,369				
					1960–1965	17.6	10.5	0.60
1965	2,785	3,510	2,947	3,722				
					1965–1970	15.7	9.5	0.61
1970	3,966	4,851	3,410	4,076				
					1970–1975	6.5	−1.4	N.A.
1975	5,852	6,696	3,630	4,019				

*The 1960 per capita personal income for New York City was estimated from the following regression equation, fitting on data for 1950, 1962, 1965–1975:

$$\underset{\text{INCOME}}{\text{NYC PER CAPITA}} = \underset{(41.3)}{219.9} + \underset{(0.009)}{0.993}\ \underset{\text{INCOME}}{\text{NYS PER CAPITA}} \qquad R^2 = 0.996 \qquad SE = 40.9$$

A corresponding equation using United States per capita income as the independent variable had an equally high R-squared, but the standard error was twice as large:

$$\underset{\text{INCOME}}{\text{NYC PER CAPITA}} = \underset{(73.9)}{642.5} + \underset{(0.019)}{1.048}\ \underset{\text{INCOME}}{\text{US PER CAPITA}} \qquad R^2 = 0.999 \qquad SE = 80.3$$

**Consumer price index for the United States and the New York-Northeastern New Jersey metropolitan area, 1967 = 100.

in real per capita personal income, has grown at a slower rate than that of the rest of the country for over *four decades*.[4]

Decline, then, despite its current status as one of the determinative political issues of the day, is not a new phenomenon. Why then has it taken so long for popular wisdom to catch up with economic reality? Why, if New York City's decline, vis-à-vis the rest of the country, has gone on for generations, has the city's economy only recently become one of those handful of critical issues that make or break a political campaign? At least four reasons come to mind.

[4] Sources: Temporary Commission on City Finances, *Economic and Demographic Trends in New York City: The Outlook for the Future*, Thirteenth Interim Report to the Mayor, May 1977; Robert E. Graham, Jr. and Edwin J. Coleman, "Metropolitan Area Incomes 1929-66," *Survey of Current Business* 48, no. 8 (August 1968): 25–48; Regional Plan Association, "The State of the Region 1977," *Regional Plan News* 101, November 1977.

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First, for most of the period during which New York City's rate of economic growth lagged behind the rest of the nation, the city's economy continued to grow. Moreover, its growth rate varied directly with the growth rate of the national economy. For example, for the 20-year period from 1950 to 1970, increases in the city's real per capita personal income remained relatively fixed at approximately 60 percent of the national per capita personal income growth rate (see Table 1). Given this linear relationship and New York's traditional role as a center of national economic activity, it is not difficult to understand why interest in the city's economy would take a back seat to discussions about the performance of the national economy.

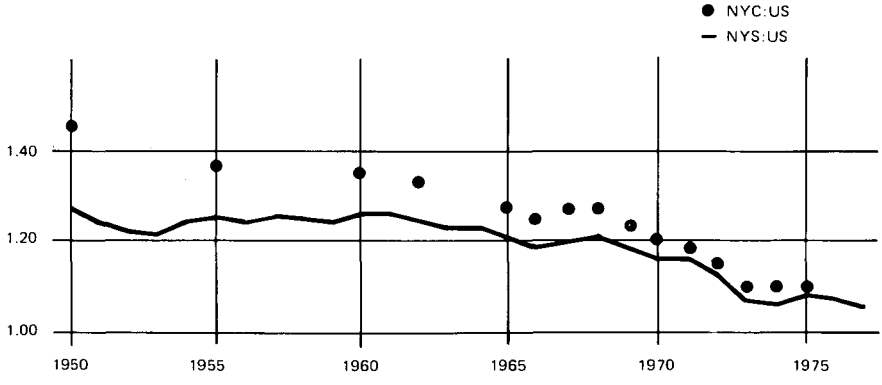
Second, despite New York City's long history of relative economic decline—or to use a more technical expression, regression toward the mean—most of its residents still considered it relatively well-off. And at first glance it was (see Figure 1). As late as 1970, the real per capita personal income for city residents was 20 percent higher than the national figure; it was not as high as it was in 1950, when it was 45 percent above the national level, but it was still significantly above the national average.

Third, those who did argue from time to time that, because of the cumulative impact of many years of less-than-average economic growth, the city's economy would someday radically alter its course for the worse, usually made their arguments in the course of challenging increases in municipal expenditures for social welfare programs. Their logic, as well as their motivation, was considered suspect by most New Yorkers who, judging by their voting behavior, held contrary views.

Fourth, New York is the prototypical New Deal city in the prototypical New Deal state. In addition to providing generous financial support for a vast menu

FIGURE 1

TRENDS IN PER CAPITA PERSONAL INCOME:
RATIO OF NEW YORK STATE TO UNITED STATES
AND NEW YORK CITY TO UNITED STATES
1950-1977
(CONSTANT DOLLARS)



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of social welfare services designed to eliminate the more pernicious effects of poverty, it has, for more than 40 years, supported candidates for national office who have advocated federally funded programs designed to stimulate wealth and resource redistribution from "richer" parts of the nation to those that are less well-off. And as a "rich" city, the polity of New York well understood that the politics of redistribution would be at its own expense. In short, a relative decline in the city's economic fortunes may have meant that the city's political philosophy was triumphant on the national level.

SOMETHING HAPPENED

But times have changed. One of the more significant changes has been a breakdown in the decades-long relationship between the growth rate of the national economy, as measured by increases in real per capita personal income, and the growth of the economy of New York City. Between 1970 and 1975, not only did the economy of New York City fail to grow at a rate equal to 60 percent of the national rate, it failed to grow at all. During this period—characterized by two major recessions and an energy crisis which is yet to be understood, much less resolved—the national real per capita personal income for the nation increased by 6.5 percent, while in New York City it went down by 1.4 percent (see Table 1). Had the city grown in tandem with the national economy, as it had for more than 20 years, its per capita personal income would have increased by 3.9 percent between 1970 and 1975 instead of declining by 1.4 percent, a 5.3 percentage point difference. Had the city's economy performed as expected between 1970 and 1975, it would have generated a total personal income in excess of $53.4 billion during 1975. The actual figure was $50.7 billion, so that the breakdown in what was apparently an ironclad structural relationship between the city's economy and that of the nation "cost" the city $2.7 billion during 1975. The cumulative "loss" in personal income for the period 1970–1975 exceeded $12 billion.

The break in this long-standing, per capita income growth relationship coincides with the precipitous drop in the number of nonagricultural, private sector jobs in New York City. During the 20-year period from 1950 to 1970, the number of these jobs remained steady at 3.1 million, creeping up slightly to a 3.2 million peak in 1969, while the rest of the state and the country were experiencing steady gains in employment. Then the 1970 recession hit, and the city never recovered. In every single year since then, the city suffered significant losses in numbers of jobs. The total eight-year loss now stands at 586,000 jobs, which represents an 18 percent drop from the 1969 peak (see Table 2).

In addition to quashing the growth of an incipient adage—"as the nation's economy goes, so goes New York City, only 40 percent slower"—the city economy's catastrophic loss of jobs and sudden deviation from its established growth patterns has caused other changes in the political and social fabric of the city.

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TABLE 2

Nonagricultural, Private Sector Employment in New York City: 1950–1977 (Thousands)*

Year	Number	Change	Year	Number	Change
1950	3,094	–	1972	2,999	−41
1955	3,086	−8	1973	2,964	−35
1960	3,130	144	1974	2,879	−85
1965	3,115	−15	1975	2,715	−164
1969	3,251	136	1976	2,686	−29
			1977	2,665	−21
1970	3,182	−69			
1971	3,040	−142		Total Since 1969 Peak	−586

*Source: *Employment and Earnings, States and Areas 1939–1975*, U.S. Department of Labor, 1977; and New York State Department of Labor.

DECLINE: PERCEPTION AND POLICY IMPLICATIONS

A major result of the 1970–1975 depression in the city's economy is that New Yorkers no longer think of their city as being relatively well-off, and not without considerable justification. In 1959, the "effective" per capita personal income for New York City residents exceeded the national average for all metropolitan areas in the nation by nearly $420, or nearly 17 percent (see Table 3). By 1969 the "effective" per capita income gap was still 14 percent, or $470. As a result of the economic slide of 1970–1975 (see Table 1), however, the city is no longer relatively well-off when compared to the rest of the nation's metropolitan areas. In 1975 the "effective" per capita income for New York City fell below that of metropolitan America for the first time (see Table 3).

The New Deal: New York's Legacy to the Nation

That New York City's "effective" per capita income in 1975 was less than the average for metropolitan America is an important observation. Even more significant, however, is the fact that New York's per capita income fell below Atlanta's 10 years ago. Atlanta's rise and New York's decline, as much as any other pair of events, symbolizes recent debate over the relationship between federal revenue and expenditure policies and local (regional) economic prosperity.

Much of the debate has taken the form, as *U.S. News and World Report* put it, of a "pork-barrel war between the states."[5] Lawmakers from New York and other industrial states experiencing economic distress contend that their states are getting too little of the federal monies spent for social, economic, and

[5] "The Pork-Barrel War Between the States," *U.S. News and World Report*, December 5, 1977, pp. 39–41.

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military purposes, while other states with healthy economies, including Georgia, have received a disproportionate share of federal expenditures. The remarks of Howard Metzenbaum (Dem., Ohio) exemplify this view: "We want equal treatment. In the past, our area has been shortchanged. And, you know, it's the squeaking wheel that gets the grease." Needless to say, Senator Metzenbaum's sentiments are shared by numerous New Yorkers.

Though an effective organizing tool, the cry for a fairer share of the federal expenditure pie obscures a point that New Yorkers, more than any other

TABLE 3

Effective Per Capita Personal Income, All SMSA Counties, New York City and Atlanta, Ga.: 1959–1975

	Current† *Dollars*	*Constant*†† *Dollars (1967 = 100)*	*Effective*††† *Dollars (1967 = 100 & US = 100)*
1959			
All SMSA Counties	$2,167	$2,482	$2,482
New York City	2,733	3,131	2,900
Atlanta, Ga	2,205	2,526	2,800
1969			
All SMSA Counties	$3,733	$3,400	$3,400
New York City	4,631	4,218	3,870
Atlanta, Ga	3,857	3,513	3,992
1974			
All SMSA Counties	$5,486	$3,714	$3,714
New York City	6,301	4,266	3,809
Atlanta, Ga.	5,777	3,911	4,298
1975			
All SMSA Counties	$5,903	$3,662	$3,662
New York City	6,696	4,154	3,644
Atlanta, Ga	6,117	3,795	4,264

†Per capita personal income data, in current dollars, are from "County and Metropolitan Area Personal Income," *Survey of Current Business,* vol. 57, no. 4, April 1977, pp. 23–48.

††Constant dollars are based on the national consumer price index as the deflator: 87.3, 109.8, 147.7 and 161.2, respectively.

†††Effective dollars deflate the constant dollars by the regional cost of living index prepared by the Bureau of Labor Statistics (see table below). The 1959 effective dollars for New York City and Atlanta are estimated using the 1967 indexes, the first year for which data are published.

Intermediate Budget Index for a 4-Person Urban Family

	1967	*1969*	*1974*	*1975*
Metropolitan United States	100	100	100	100
New York-Northeastern N.J.	108	109	112	114
Atlanta, Ga.	90	88	91	89

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Americans, should not forget. For more than 40 years the state of Georgia (as well as virtually all of the Southern states) has been one of the major beneficiaries of national economic policies designed to promote wealth and resource redistribution from rich parts of the country to poorer regions. These policies have been firmly supported by New York. That the major metropolitan community (Atlanta) in a state thought sufficiently poor to merit preferential economic treatment should now appear to be wealthier than New York City, the incubator of these preferential economic policies, is a "victory" for New York and its major political contribution to the nation, the New Deal. But what price victory?

F.D.R. AND THE NEW DEAL

Franklin D. Roosevelt, as Frank Freidel reminds us in his marvelous short book, *F.D.R. and the South*, was "a New Yorker who liked to think of himself as also a Georgian by adoption."[6] During his long periods of residence at Warm Springs, Georgia—where he first came in 1924 to carry out his fight against the ravages of poliomyelitis and to nurture the political popularity he had earned as the 1920 Democratic candidate for vice-president—he got to know the South as few Northerners of his period, party, and class ever did.

Roosevelt was especially sensitive to the grinding poverty of the South and the central role that agrarianism played in its perpetuation. During the First World War, farmers were encouraged by the federal government to increase their production drastically. To finance this expansion "many a farmer had gone recklessly into debt and was therefore particularly vulnerable to the collapse of the foreign market which immediately followed the war."[7] Farm prices plunged so drastically that the agrarian South never reaped any benefits from the booming national economy of the 1920s. In fact, many years before the stock market crash of 1929, Roosevelt could see that the region was in a state of severe economic depression.

When Roosevelt finally assumed office, it came as no surprise that he favored the South in fashioning his program for postdepression economic recovery. If he had an obligation to get the country moving again, he felt a special obligation to get the rural South moving a bit faster than other regions.

Fifteen major laws designed to promote economic recovery were railroaded through Congress during Roosevelt's first 100 days as president. In keeping with Roosevelt's commitment to aid the economically devastated solid South, a number of these laws were aimed at halting the 12-year decline in American agriculture: (1) the Emergency Banking Act made possible the execution of inflationary budgetary policies, a position that had been pushed by senators and congressmen from heavily agricultural states throughout the 1920s; (2) the

[6] Frank Freidel, *F. D. R. and the South* (Baton Rouge: Louisiana State University, 1965), p. 1.

[7] Kenneth S. Davis, *F. D. R.: The Beckoning of Destiny* (New York: G. P. Putnam's Sons, 1971), p. 632.

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Civilian Conservation Corps, though ostensibly designed to aid unemployed young male residents of the nation's cities, provided direct economic assistance to rural America through programs of conservation and soil reclamation; (3) the Federal Emergency Relief Act established a national relief system which distributed one-half of its appropriation to the states on the basis of need—a radical concept which, needless to say, favored the more needy South; (4) the Agricultural Adjustment Act inaugurated a national agricultural policy that was a key element in the plan for Southern economic recovery since the South, with 21.3 percent of the nation's labor force, contained 41.4 percent of the nation's agricultural workers; (5) the Emergency Farm Mortgage Act provided for the refinancing of farm mortgages; (6) the Tennessee Valley Authority Act offered low-cost power and comprehensive economic planning assistance; (7) the Farm Credit Act reorganized agricultural credit activities.

Roosevelt's commitment to the South reflected something more than the ephemeral promise of an aspiring politician paying a political debt. In 1938, six years into his presidency, and two years before the "end" of his tenure (the possibility of a third term had not yet been raised), Roosevelt still clung to the notion that the nation owed the South preferential economic treatment. Responding to the National Emergency Council's *Report on Economic Conditions of the South*, Roosevelt said:

> It is my conviction that the South presents right now the nation's No. 1 economic problem—the nation's problem, not merely the South's. For we have an economic unbalance in the nation as a whole, due to this very condition of the South's. It is an unbalance that can and must be righted, for the sake of the South and of the Nation.[8]

The New Deal permanently changed the nature of the American commonwealth, converting its government from a passive monitor of social and economic trends to an active participant in the marketplace "on the side of the people's interest." In no other region of the country, however, did the New Deal have as profound an impact on social thought and political debate as it did in the South.

A LIMITED PARTNERSHIP WITH THE NATIONAL GOVERNMENT

Throughout the agricultural depression of the 1920s, Southern intellectuals and politicians argued vociferously among themselves over the economic future of the South. That the South was experiencing economic stagnation while Northern industrial states were enjoying the fruits of the booming 1920s made allegations of Northern economic exploitation all the more credible. It also made social and political strategies that would isolate the Southern way of life from the contaminating influence of the "teeming hellholes of the industrial north" all the more attractive to Southerners.

The unique civilization the Southerner fought for, of course, was grounded in the economic subjugation and political disenfranchisement of its black in-

[8] Freidel, *F. D. R.*, p. 99.

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habitants, who in 1930 constituted more than 30 percent of the population in most of the Southern states. The politics of white supremacy found its most effective outlet in the Ku Klux Klan (KKK).[9] The KKK, viciously racist, anti-Catholic, and belligerently hostile to the social forces accompanying urbanization, dominated Southern politics throughout the 1920s; its staunchest supporters came from agrarian communities in which blacks constituted up to 80 percent of the local population.

Juxtaposed to those large numbers of people who wanted to maintain the Southern way of life—the KKK being only the extreme manifestation of this sentiment—were those who felt that the South could prosper only if it entered into a limited partnership with the national government, one that would improve the South's economy through moderate industrialization, the adoption of inflationary economic policies, and efforts to introduce modern technology to Southern agriculture. Though these "progressives" did not support the politics or tactics of the KKK, they too loved the Southern way of life, supported its racial codes, and shared its romantic view of small town life and agrarianism. But they also knew that the road to economic parity with the North required basic changes in the relationship between the South and the national government. If these changes involved the risk of Northern interference with the Southern way of life, then it was a risk worth taking.

It was the New Deal, the program of a New Yorker who was an honorary Georgian elected with the support of the solid South, that ultimately resolved the debate over the future economic role of the South. The South was so painfully in need of succor that its politicians had no choice but to embrace the New Deal and its underlying threat of federal interference with a sacred lifestyle:

> . . . the Roosevelt measures represented a giant, nationwide cornucopia from which federal aid poured into the desperately Depression-ridden South. Distressing though this aid was to many of the Southern elite, Southern politicians concerned themselves more with channeling, rather than stemming, the flow. They did not want to join Republicans in curtailing it altogether. Never previously had so much been done for the South.[10]

As the money passed from Northern to Southern hands, the die was cast. Although aspects of the New Deal were consistently and bitterly challenged by conservative Southern politicians—especially those who had personally capitalized upon the separate and unequal role of the South in the national economy and the subservient role of cheap black labor in the South's economy—the positive relationship between federal aid and Southern economic growth soon became axiomatic. The South had land, raw materials, labor, and disproportionate influence in the Congress, but little access to capital for investment. The federal government was not only in a position to supply this capital, but

[9] V.O. Key, Jr., *Southern Politics in State and Nation* (New York: Alfred A. Knopf, 1949).
[10] Frank Freidel, *F. D. R.: Launching the New Deal* (Boston: Little, Brown & Co., 1973), p. 99.

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also "professed" to be just a bit more responsive to the special mores of the South than were Northern bankers.

THE SOUTHERN CHANNEL AND THE FLOW OF FEDERAL FUNDS

The progressive character of the income tax system, the central role of federal subsidies in the New Deal's agricultural policies, and a host of national social welfare initiatives (providing higher per capita aid to states which had the highest percentages of the poor, sick, and aged) guaranteed that the South would get back more from the national treasury than it contributed in the form of federal tax payments. In essence, that is what the New Deal was all about. But those funds were small solace for great adversity; they reflected a history of poverty and "exploitation" that many Southerners were determined to put behind them. In order to build its economy, to turn itself around, to grow at a faster rate than the rest of the country, the South required much more than generous welfare payments.

During this era, Southern members of Congress exercised almost total veto power over any piece of legislation they wished to kill. Armed with their "new understanding" of economic reality, they pursued the politics of redistribution with a vengeance. Their first target was a natural one. In exchange for the support they gave to Roosevelt and his efforts to prepare the nation for possible involvement in the great war that was engulfing Europe at the end of the 1930s, they demanded that the South participate in every phase of the war effort, and then some. They wanted to make sure that the World War I economic experience—a short-term boom followed by a long-term bust—would not be repeated. Defense dollars should be used, they insisted, as a vehicle for economic development. And they got just what they wanted.

To illustrate the favorable treatment given to the South during World War II and after, we will focus on federal military wage and salary disbursements between 1940 and 1976, comparing data for Georgia, our prototypical Southern

TABLE 4

Cumulative Federal Military Wage and Salary Disbursements: 1940–1976 (Millions of Dollars)*

	United States		*New York*		*Georgia*	
	Amount	*Percent*	*Amount*	*Percent*	*Amount*	*Percent*
1940–1950	$ 66,565	100.0%	$ 4,000	6.0%	$ 2,180	3.3%
1951–1960	78,442	100.0	3,099	4.0	2,791	3.6
1961–1970	115,871	100.0	2,983	2.6	5,155	4.4
1971–1976	103,376	100.0	2,225	2.2	3,349	3.3
Total	$364,254	100.0%	$12,307	3.4%	$13,475	3.7%

* *Survey of Current Business,* various years.

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state, and New York, our model industrial state. During this period, the federal government expended more than $364 billion for military salaries. Of this nationwide total, 3.4 percent was spent in New York and 3.7 percent in Georgia (see Table 4).

How would these cumulative expenditures for military wages and salaries have been distributed if they had been allocated on a per capita basis? Using a population per capita allocation scheme, New York State would have "received" more than $21 billion (in current 1940–1976 dollars) above the amount that it actually received. And Georgia would have "lost" $5 billion.

These "gains" and "losses" represent only the tip of the iceberg. When one takes into account the stimulative effects of the capital investment that the federal government made in constructing military installations in Georgia, the additional federal "impact aid" that Georgia received because of its large military population, and the multiplier effect of the wages and salaries of military personnel, the magnitude of gains and losses significantly increases. Taking into account these multiplier effects and the impact of inflation, one would not be too far off in estimating gains and losses at four times the values computed above (Table 5).

TABLE 5

Hypothetical Cumulative Federal Military Wage and Salary Disbursements: 1940–1976 (Millions of Dollars)

	United States	*New York*	*Georgia*
Hypothetical Allocation in Proportion to Population			
1940–1950	$ 66,565	$ 6,657	$1,543
1951–1960	78,442	7,516	1,755
1961–1970	115,871	10,601	2,586
1971–1976	103,376	8,981	2,366
Total	$364,254	$33,755	$8,250
Actual Gain or Loss (–) Compared to Hypothetical			
1940–1950	$ 0	$–2,657	$ 637
1951–1960	0	–4,417	1,036
1961–1970	0	–7,618	2,569
1971–1976	0	–6,756	983
Total	$ 0	$–21,448	$5,225

Though no causal relationship can be proved, it is interesting to note that the average annual per capita personal income growth rates between 1940 and 1976 (in constant dollars) for New York and Georgia are highly correlated with the per capita cumulative federal military wage and salary disbursements these states received during the same period. New York had lower-than-average military wage and salary disbursements, and also lower-than-average income growth. The opposite was true for Georgia (Table 6).

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TABLE 6

Index of Per Capita Cumulative Federal Military Wage and Salary Disbursements and Per Capita Personal Income Growth * *Rates (United States = 100)*

	1940–1950	*1950–1960*	*1960–1970*	*1970–1976*
United States				
Military Wages & Salaries	100	100	100	100
Personal Income Growth	100	100	100	100
New York				
Military Wages & Salaries	60	41	28	25
Personal Income Growth	41	90	91	38
Georgia				
Military Wages & Salaries	141	159	199	142
Personal Income Growth	176	150	139	114

*Computed from data published in *Survey of Current Business,* various years.

PRE- AND POST-DEPRESSION PER CAPITA INCOME GROWTH RATES

Below is a table containing information on per capita income for the United States, New York, and Georgia for 1920 (a decade before the Depression), 1929, 1933 (the first year of the New Deal), and 1940 (the beginning of the United States' buildup for World War II). Nationally, per capita income rose during the boom decade following World War I, was cut nearly in half by the Depression, and then rose smartly during the New Deal. Both New York and Georgia followed this general pattern, but there were significant differences (see Table 7). During the boom of the 1920s, New York's per capita income grew nearly twice as fast as the national average, while Georgia's per capita income grew hardly at all. During the Depression, income in New York fell faster than in Georgia, but during the New Deal, income grew much faster in Georgia than in New York.

The faster rise of per capita income in Georgia compared to New York typifies the differential regional impact of New Deal economic recovery policies. In the decade just before the 1929 depression, per capita personal income in the industrial Mideast (i.e., Mid-Atlantic States) and Great Lakes regions grew at a rate faster than the overall average for the nation. New York State's rate was even greater than the growth rate for the Mideast region. Georgia's growth rate was some 14 percent slower than the nation's.

After the depression, these relationships were reversed, and they have remained so until this very day. Since 1929, the per capita personal income growth rate for residents of the Mideast has continuously lagged behind the growth rate for the rest of the country. With the exception of the decade from 1959 to 1969, New York's per capita income growth rate has continuously lagged behind the overall growth rate of its own region, the Mideast.

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TABLE 7

Trends in Per Capita Income for the United States, New York, and Georgia: 1920–1940

	Per Capita Income (in dollars)			
	1920†	*1929*††	*1933*†††	*1940*††
United States	$ 658	$ 705	$375	$592
New York	1,026	1,164	634	871
Georgia	348	349	204	336

	Changes in Per Capita Income					
	1920–1929		*1929–1933*		*1933–1940*	
	Amount	*Percent*	*Amount*	*Percent*	*Amount*	*Percent*
United States	$ 47	7%	$-330	−47%	$217	58%
New York	138	13	−530	−46	237	37
Georgia	1	*	−145	−42	132	65

*Less than 0.05 percent.

†1920 data are from Harvey S. Perloff, et al., *Regions, Resources and Economic Growth* (Baltimore: Johns Hopkins Press, 1960), p. 650.

††1929 and 1940 data are from Robert B. Bretzfelder, "Sensitivity of State and Regional Income to National Business Cycles," *Survey of Current Business,* April 1973, Table C.

†††1933 data are from Charles F. Schwartz and Robert E. Graham, Jr., "Personal Income by States Since 1929," supplement to the *Survey of Current Business,* September 1956.

The Southeast is the *only* region whose per capita income growth rate has consistently outpaced that of the rest of the nation for the entire post-Depression period. Between 1940 and 1976, real per capita income for Southeasterners went from $1,101 to $4,178, an increase of 279 percent. Georgia's per capita income increased from $1,091 to $4,198, an increase of 285 percent. During the same period, per capita income for Mideasterners increased from $2,565 to $5,256, an increase of 105 percent. New York's per capita income increased from $2,828 to $5,350, an increase of 89 percent. For the periods 1949–1959, 1959–1969, and 1969–1976, the average annual growth rates in per capita income for the Southeast exceeded that of the Mideast by 40, 41, and 107 percent, respectively. For the same period, the average annual per capita income growth rate for Georgia exceeded that of New York by even larger margins (see Table 8).

It would be fatuous to attribute the rapid economic expansion of the South entirely to the economic policies and social welfare programs of the New Deal. Industrial and commercial expansion occurred in the South primarily because of growth in the country as a whole. In addition, other factors contributed to the South's economic boom. Firms faced with the need to rehabilitate plants and machinery in the traditional industrial states determined that it was more profitable to build new production facilities where the climate was better, taxes and energy costs lower, and there were fewer potential labor difficulties.

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Undoubtedly, wage differentials played a decisive role for marginal, labor-intensive industries such as the apparel trades, light electronics, and furniture manufacturing. Others were attracted to the South through a combination of vigorous promotion campaigns, generous tax incentive programs, and free or inexpensive industrial facilities.

Still, one should not forget that during the New Deal, the "poor" states of the region received federal assistance far out of proportion to the federal tax revenues they generated. And this imbalance was financed, to a large degree, by the negative net revenue flow between "rich" parts of the country (like New York) and the federal fisc. And here lie the seeds of revolution.

New York against the New Deal?

Given New York's current economic difficulties and the growing belief that the city's problems may in part be the result of disadvantageous federal expenditure policies dating from the New Deal, how long will it be before New Yorkers come to the conclusion that continued adherence to the New Deal ideology in national affairs is counterproductive to the economic interests of New York? How long will it be before the New York City congressional delegation evaluates every major spending bill strictly in terms of its impact on the "balance of trade" between Washington and New York? In other words, is it possible that New

TABLE 8

*Average Annual Growth Rate of Per Capita Personal Income Constant Dollars (1972 = 100)**

	Growth Rate					Ratio		
Period	*United States*	*South-east*	*Georgia*	*Mid-east*	*New York*	*Georgia to U.S.*	*New York to U.S.*	*Georgia to N.Y.*
1920–1929†	2.9%	2.1%	2.2%	3.4%	3.6%	0.8	1.2	0.6
1929–1940††	−0.2	0.6	1.0	−0.6	−1.3	**	**	**
1940–1949††	2.9	5.0	5.1	1.4	1.2	1.8	0.4	2.4
1949–1959††	2.0	2.8	3.0	2.0	1.8	1.5	0.9	1.7
1959–1969††	3.3	4.1	4.6	2.9	3.0	1.4	0.9	1.5
1969–1976†††	2.1	3.1	2.4	1.5	0.8	1.4	0.4	3.0

*Current dollars were deflated by the GNP implicit price deflator for personal consumption expenditures, 1972 = 100. *Survey of Current Business,* various years.

**Not applicable.

†1920 data are from Harvey S. Perloff, et al., *Regions, Resources and Economic Development* (Baltimore: Johns Hopkins Press, 1960), pp. 27 and 650.

††Data for 1929 through 1969 are from Robert B. Bretzfelder, "Sensitivity of State and Regional Income to National Business Cycles," *Survey of Current Business,* vol. 53, no. 4, April 1973, pp. 22–33, reprint including supplemental material.

†††Data for 1976 are from Robert B. Bretzfelder, "State Personal Income: 1975: I–1976: IV," *Survey of Current Business,* vol. 57, no. 4, April 1977, pp. 18–22.

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York will eventually feel forced to renege on its long-standing commitment to economic policies and social welfare programs that originated during the New Deal?

New York's Senator Daniel Patrick Moynihan thinks that this possibility is not as farfetched as it may appear at first glance. Arguing that "the federal government is, however unintentionally, deflating the economy of New York in order to sustain expansion elsewhere" through "sustained redistribution of resources away from New York and the northeast quadrant," Moynihan concludes:

> The crucial point is that the redistributional mode of government, with its bias against New York is now firmly established in Washington. It is sustained by the rewards it provides to others.
>
> What influence can we bring to bear on our own behalf? The answer would seem that the only serious sanction we have is the threat to withdraw our support of this mode of government. This, in my view, would be a huge loss to the nation. But if the nation does not listen, it could turn out to be the only way to save ourselves.[11]

If Moynihan is right, then the price that New York would have to pay to save itself goes far beyond the repudiation of the policies instituted by Franklin D. Roosevelt and his New Deal. Paradoxically, in order to save its own skin, New York would have to repudiate its rightful heritage. For the New Deal was as much the natural outgrowth of the "cosmopolitan liberalism" of New York City as it was the program of any one individual.

COSMOPOLITAN LIBERALISM

By "cosmopolitan liberalism" we mean the complex of social, economic, and political positions taken by "urban new stock lawmakers"[12] and reform-minded progressives (usually native, upper middle class) from New York City during the three decades preceding the election of Roosevelt as governor of New York State in 1928. In the main, the "liberalism" of the immigrant political leader stemmed from the day-to-day problems faced by his constituents, while that of the old stock progressives was more a function of *noblesse oblige*.

Lacking skills and feared by native workers as a threat to their employment security, male immigrants to New York's teeming ethnic enclaves were usually the "last hired and first fired." What work they could find was frequently dangerous, monotonous, and poorly compensated. If they were ill, injured, or laid off, these "foreign-stock" workers found themselves without adequate income, and wives and children were often forced into the labor market. The customs and religious beliefs of urban immigrants were also subjected to stringent attack from many quarters of the native population. Some wanted to ostracize

[11] Daniel Patrick Moynihan, "The Senator Explains His Point," *New York Times*, August 4, 1977.

[12] The term is John D. Buenker's, whose excellent study, *Urban Liberalism and Progressive Reform* (New York: Charles Scribner's Sons, 1973), we will make extensive use of throughout this section.

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them completely from polite society, while others sought to make them over in the image of the refined English Protestant.

It is not surprising that the leadership of the "urban new stock lawmakers" in New York City—the Irish-dominated Tammany Hall—chose to focus on "bread and butter" liberalism. They promoted programs and policies marked by "government intervention in the economy to protect the less fortunate, the welfare state, a tax policy based upon the ability to pay, a 'one man one vote' political philosophy and a determined opposition to legislated conformity and morality."[13] The progressives directed their attention "to the intolerable conditions created by ever-spreading industrialism, to the devouring of the nation's natural resources by the ever growing trusts, to the incredible corruption that characterized government at all levels."[14]

Typical of those who flocked to the progressive standard was Franklin D. Roosevelt, elected to the New York State Senate in 1910. The scion of an old New York family, Roosevelt won immediate acceptance as a major spokesman for the progressive and upstate wings of the Democratic party. In addition to working for a wide variety of agricultural and conservation measures, he strongly supported the progressive legislation proposed by his counterparts in the city: a workman's compensation law, limitation of work hours for women, and creation of a state labor department. As Frances Perkins (who served in Roosevelt's cabinet as secretary of labor) later pointed out, this program of labor legislation, forged by the progressive-Tammany Hall coalition, was extraordinary for the effect that it had on the development of New York's political consciousness.

> The extent to which this legislation in New York marked a change in American political attitudes and policies toward social responsibility can scarcely be overrated. It was . . . a turning point; it was not only successful in effecting practical remedies but, surprisingly, it proved to be successful also in vote getting. . . . The fact that the Democratic party became dominant in New York for many years largely on the basis of this program of legislation (combined with competent, sympathetic administration), riveted in American life the conception that it was the duty and opportunity of people elected to office to develop programs for prevention of poverty and for improving the conditions of life and work of all people.[15]

Roosevelt spent only three years in the state senate, leaving in 1913 to join the Wilson administration as assistant secretary of the navy. But the relationships he formed with Robert F. Wagner, Tammany Hall "boss" Charles F. Murphy, and other practitioners of the emerging "cosmopolitan liberalism" were instrumental in the formulation of his progressive philosophy of economics.[16] Of

[13] Ibid., p. viii.

[14] Milton Viorst, *Liberalism: A Guide to Its Past, Present and Future in American Politics* (New York: Avon Books, 1963), p. 42.

[15] Frances Perkins, *The Roosevelt I Knew* (New York: Harper & Row, 1946), pp. 23–24.

[16] See Daniel R. Fusfeld, *The Economic Thought of Franklin D. Roosevelt and the Origins of the New Deal* (New York: Columbia University Press, 1956).

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particular importance was the influence of Roosevelt's political mentor, Alfred E. Smith.

Before and after Roosevelt left New York for his position in the Wilson administration, Smith led the struggle for a new political ideology and a government that would exercise its power for the masses. First as Democratic leader of the state assembly (1910–1915) and later as governor (1918–1920 and 1922–1928), Smith built a record that made New York the national leader in social legislation. With Smith at the helm, the progressive-Tammany Hall coalition pressed for a wide range of welfare legislation: rent control; public housing; regulation of the milk industry; expanded public health facilities; increases in state aid to education; improvements in workmen's compensation and factory codes; and ratification of the federal child labor amendment.[17] In addition to this formidable array of social welfare legislation, Smith shepherded through what then amounted to major (if not radical) innovations in state finance: a general income tax on individuals and corporations; and the use of bond issues to raise revenue for capital improvements such as parks and hospitals.

Of course, the coalition between progressives and Tammany Hall (which Smith dominated after the death of Charles F. Murphy in 1925) did not always function perfectly. At times the alliance was uneasy; despite Smith's personal integrity, ethnocentric machine politics were still anathema to most "true American" New York progressives. And yet, a working alliance was forged between a major urban political machine and a loosely bound group of progressives that made New York unique among the industrial states. Other states such as Ohio, Illinois, and Pennsylvania also boasted large urban and ethnically heterogeneous populations. They all had their counterparts to Tammany Hall, but none of their political machines supported sweeping reform measures comparable to those backed by the Smith-Murphy, Tammany-based New York Democratic party of the 1920s.

Prior to 1932, despite its status as the most populous state in the nation—the one having the country's largest city and most powerful and effective political machine—New York had no special national significance apart from the reputation of Wall Street.

And to the degree that New York's unique status was the achievement of any one individual, it was Al Smith's, about whom H. L. Mencken said, "his world begins at Coney Island and ends at Buffalo." Though Smith was quintessentially a New York politician, he stood for something that would soon capture the imagination of the entire nation.

> Smith had been a brilliant reform governor—the only brilliant reform governor anywhere in the era of Coolidge Republicanism—and he had been the first representative of non-Anglo-Saxon American to win the leadership of a major party. The Great Crash, arriving in the middle of the Bootleg era, came as the ultimate defeat for the Protestant Republic, which was persisting a-religiously in the form of Hoover's American System. What the alternative to the American System would be, nobody

[17] Ibid., pp. 92–93.

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knew, but the obvious place to look for it was in Smith's Democratic party and in Smith's state of New York.[18]

The "cosmopolitan liberalism" that had been forged by the politics of compassion and enlightened self-interest was given its day. And not a moment too soon. In 1932, America stood on the brink of total disaster. Its institutions were under attack from every quarter, its leadership suspect, and its economic policies in a state of chaos. More than 25 percent of its workers were unemployed. Distribution of wealth was severely imbalanced among regions as well as among the populace. The South's economic depression was rounding out its first decade, and American agriculture was on the brink of collapse as real farm income dropped below 1910 levels. The national government—hanging on to its belief in laissez-faire capitalism, a self-adjusting economy, and rugged individualism—was immobilized. America needed new leadership, a new mode of government, and a new ideology. New York, and especially New York City, provided these things. H. L. Mencken, in a 1927 article, wrote the following description of Al Smith's New York City:

> For years New York City has been gradually sliding away from the rest of the country, and today it is almost as much foreign soil as Paris or Warsaw. In ideas as in manners it is the complete antithesis of the Middle West, the West and the South. What Kansas or Tennessee or Utah venerates, New York laughs at. What New York esteems is diabolical to Kansas, Tennessee and Utah. This split, it seems to me has been productive of much good. It has made New York a refuge for civilized Americans, and so saved them to the country.[19]

However by 1932, the heart of the Depression, things had changed. With a "mode" of government produced by an idiosyncratic amalgam of constituencies, New York was closer to the American mainstream than Mencken would have thought possible only five years earlier. As Gilman M. Ostrander points out:

> Barring some dreadful political accident, 1932 was destined to be New York's year in Democratic politics, and the rest of the decade was destined to be New York's decade in national politics generally. The nation looked to New York for leadership in 1932, and New York responded by giving the nation, not only Roosevelt, but most of the political makings of the New Deal as well. Roosevelt became the formulator of the New Deal—"Dr. New Deal," as he himself put it—but it had been New York's qualifications for national leadership, rather than FDR's own personal ones, which had originally won him the office, and it was New York's political ideology, rather than his own, which became the New Deal. New York is not America, as the saying goes, but during the New Deal era it came nearer to being America than ever before or since.[20]

[18] H. L. Mencken, *On Politics: A Carnival of Buncome* (New York: Vintage Books Inc., 1956), p. 146.

[19] Gilman M. Ostrander, *American Civilization in the First Machine Age, 1890-1940* (New York: Harper & Row, 1970), p. 318.

[20] Ibid., p. 319.

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But if 1932 was New York's finest hour, then today must surely be its most troubled. Whenever politicians need to make a point about the failure of "cosmopolitan liberalism," or unleash philippics against the "welfare state," New York City serves as the perfect object lesson. Its burgeoning welfare rolls, high unemployment rates, abandoned neighborhoods, frightening crime statistics, runaway manufacturing enterprises, fleeing middle class, powerful municipal unions, and empty treasury are all heralded as the inevitable outcome of the city's traditional support for macroeconomic policies and social welfare programs designed to foster wealth and resource redistribution. They rant that the city is now suffering from the worst of all sins—the establishment of objectives that were impossible to achieve.

To those who believe that the city's economic decline is largely the result of its own cosmopolitan liberalism, Senator Moynihan's threat to withhold New York's support for the New Deal "mode" of government drips with irony. But to those who believe that a breakdown in the seemingly ironclad structural relationship between national economic growth and the health of the New York economy is a far more important determinant than the failure of the city's polity, then the arguments of Senator Moynihan are like a revolutionary call to action.

For three years, an almost Sisyphean battle has been waged against municipal bankruptcy, largely because of the belief that whatever ails New York can be cured by New Yorkers; "if they have the will to make short-term sacrifices necessary to undo the sins resulting from past municipal profligacy." But leaders of the city's financial and political community have begun to pay more attention to the role that external forces have played in the city's economic decline. Had Moynihan put forth his arguments three years ago, they would have been dismissed as self-serving, a characteristic lament designed to avert long overdue reforms in the financial operations of the city. This is no longer the case. In fact, Moynihan has received unexpected support for his contentions from important policymakers at the federal level.

In a recent letter to Moynihan, Charles L. Schultze and Stuart Eizenstat, two of President Carter's chief advisers on economic and domestic affairs, acknowledged the existence of a net negative revenue flow between New York State and the national treasury.

> The principal reasons for New York's "deficit" is that its Federal tax payments are high, not that Federal expenditures in New York State are low. Tax payments are high because New York is a high income state . . . and our tax system is progressive.
>
> New York State does fall short in per capita measures for those expenditure categories—natural resources, highways, water projects—for which it may have a lesser need per capita than other states because of its geographic and demographic characteristics.
>
> We do not agree that the Federal Government needs more systematic information on the overall impact of all of its programs, collectively, on regional economies.
>
> New York State is clearly disadvantaged by particular distribution formulae which put too much weight on *average* income, since this single statistic does not adequately

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> reflect New York's very unequal distribution of income around the mean. Formulae certainly should take into account, in a sensible way, of such factors as poverty, congestion, regional costs and other specific considerations appropriate to particular programs.[21]

Though also acknowledging the existence of a negative net revenue flow between New York and the federal government, Secretary of Commerce Juanita M. Kreps has justified this situation in terms of the prevailing New Deal ideology of cosmopolitan liberalism.

> . . . I think we all recognize that inequality of tax payments and receipts of Federal outlays by individual States has to be expected. Much of the enabling legislation of current programs is aimed at alleviating the plight of the poorest segment of the Nation's citizens. New York is a State with relatively high personal and business income subject to Federal taxation. Some States must be net taxpayers if others are to be funded, the wealthier States are the logical source for a larger share of the necessary tax receipts. I agree, however, that consideration should be given to using living costs as a factor in allocating the available program resources. We should keep in mind, though, that at least part of the pressure on living costs stems from the scope of the actions for services to constituents adopted by the States themselves.[22]

Where will this new willingness to question the impact of federal revenue and expenditure policies lead? Will New York, the most cosmopolitan city in the nation, a city which once produced a set of political principles broad enough to win the support of almost the entire country, attempt to seek succor from the federal fisc through policies based on economic regionalism and political provincialism? Will New York legislators withdraw support for traditional national fiscal and monetary (macroeconomic) policies designed to lower unemployment and inflation rates on the grounds that they are insufficiently sensitive to the peculiar problems of New York City? Will New Yorkers eventually accept the proposition that a national ideology of cosmopolitan liberalism is the cause of New York's economic ossification? In short, will New Yorkers come to see the federal government as a counterproductive force that, having had the good sense to adopt New York's ideology of wealth and resource redistribution four decades ago, proceeded to "pay off" its debt to New York by failing to take into account the *cumulative* impact of national economic and social welfare programs that were, despite the best of intentions, biased against rich cities like New York?

The fact that these questions are now entertained—and with a high degree of seriousness—by those who have always pointed out with pride to New York's tradition of cosmopolitan liberalism, is tantamount to a political revolution. And the answers to these questions will shape not only the politics of New York during the next decade, but also the politics of the nation.

[21] Letter to Senator Daniel P. Moynihan from Charles L. Schultze (Chairman of the Council of Economic Advisers) and Stuart Eizenstat (Assistant to the President for Domestic Affairs and Policy), dated September 22, 1977.

[22] Letter to Senator Daniel P. Moynihan from Juanita M. Kreps (Secretary of Commerce), dated August 5, 1977.

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CITY-STATE FISCAL RELATIONSHIPS

Up to now, we have focused on the fiscal relationship between New York City and the federal government. But there is another intergovernmental fiscal relationship that is equally important and problematic to the city. Just as some contend that federal revenue and expenditure policies have had an adverse impact on the city's economy, others have raised questions about the equity of New York State's revenue generation and disbursement policies. We will briefly touch on just two areas that are being closely scrutinized: education and social services.

The State of New York has an extensive local assistance program. In fiscal 1976, $6 billion (half the state's budget) was distributed for education, social services, housing, health, transportation, revenue sharing, and other purposes. New York City received nearly half of these local assistance grants, and since the city contains only 42 percent of the state's population, it appears that New York is doing fairly well compared to the rest of the state. However, critics contend that for the two areas of greatest need, education and social services, the city should receive more.

New York's system for financing elementary and secondary public education —the method used annually to raise and distribute $8 billion to more than 700 local school districts enrolling 3.4 million pupils throughout the state—has been challenged in the lawsuit *Levittown* v. *Nyquist*. Currently pending decision after nine months of proceedings in the state trial court, this lawsuit seems to be moving the state swiftly and inexorably towards a major constitutional crisis.

As plaintiffs-intervenors in the *Levittown* suit, the state's four largest cities—New York City, Buffalo, Rochester, and Syracuse—have complained that the formulae for distributing state aid to education ignore the fiscal burdens and educational problems unique to urban localities. According to the intervenors, the formulae treat the large cities as if they were "wealthy" and therefore less deserving of aid. In fact, they argue, the city school districts do not have adequate resources for their public education programs, and the state is imposing an unfair burden on the urban tax base by failing to provide enough financial assistance.[23]

The argument concerning social services is slightly different. The state requires all counties to "share" 25 percent of the cost of public assistance programs and medicaid. Critics of this arrangement point out that since New York City is coterminous with its constituent counties and has no "affluent" suburban ring, it is the *only* locality that must shoulder the entire cost of the 25 percent local share. The disproportionate burden that this places on the city's tax base is further exacerbated by the fact that 70 percent of all public assistance recipients in the state live in New York City.

[23] See Bernard R. Gifford, "Financing Elementary and Secondary Education in New York State: Levittown v. Nyquist and the Coming Constitutional Crisis" (Draft) (New York: Russell Sage Foundation, November 1977).

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One measure of the extreme disparity between the city's burden and that of the rest of the state is the per capita cost of the local share of public assistance and medicaid. In 1975, New York City's per capita cost was $199, compared to only $28 in the rest of the state. In the aggregate, this means that the city was mandated to pay for 75 percent of the statewide local share of public assistance and medicaid. Other localities, with 58 percent of the state's population and 30 percent of all public assistance recipients, had to pay for only 25 percent of all local costs.[24]

New York State's system for financing schools and social services is based on the proposition that New York City is significantly richer than the rest of the state and is therefore able to provide disproportionate support for these income redistribution programs. The city argues that it is carrying much more than its fair share of the fiscal burden, and a major reassessment of these basic assumptions is long overdue.

THE END OF THE "SOCIAL WELFARE CITY"

There can be no doubt that notions of what New York City's posture vis-à-vis the federal and state governments should be will undergo close scrutiny and fundamental changes over the next decade. The New York City Seasonal Financing Act of 1975 provides $2.3 billion in short-term loans to the city, and the New York State Legislature created the Municipal Assistance Corporation and the Emergency Financial Control Board to plan, monitor, and exercise ultimate control over the city's finances. The city is locked into a continuing struggle to avert *de jure* bankruptcy, whose potential impact on the national and international economy could be devastating. Given all of the formal machinery as well as the anguish, it is improbable that the city's fiscal and managerial practices—as well as the state of the city's economy—will ever again be a subject relegated to the back pages of daily newspapers, the parlors of Wall Street, or the halls of academia.

But however the debate on intergovernmental fiscal affairs proceeds, it is highly unlikely that the city's polity will ever again accept as social gospel the dictates of traditional cosmopolitan liberalism. For no matter what the outcome of this debate, one basic fact cannot be ignored: the breakdown in the relationship between the city's economic growth and the health of the national economy was preceded by unprecedented increases in federal and state grants-in-aid to the city.

In 1961, federal aid, then totaling $110 million, constituted only 4 percent of the city's total revenues. By 1976, the federal contribution jumped to $2.5 billion, or 22 percent of the total budget. The increase in state aid has been less dramatic, but nevertheless significant. In 1961, revenues from state sources totaled

[24] Temporary Commission on City Finances, *The Role of Intergovernmental Fiscal Relations in New York City*, Fourteenth Interim Report to the Mayor, May 1977, p. 61.

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$455 million, or 19 percent of the total city revenue base. By 1976, state aid hit the $3.2 billion mark and constituted 28 percent of the city's operating revenues (see Table 9).

For more than 40 years, the city endorsed a municipal government that exercised its power, including the power to tax, for the benefit of poor and lower-middle-class residents. When an occasional alarm was sounded over New York's lag in economic growth, the problem was usually cast in terms of the city's ability to continue *expanding*, at previous rates, a wide variety of social welfare services: free tuition at the municipal colleges; rent control; a vast municipal hospital system; a relatively generous and unintimidating welfare system; subsidized housing for the middle class; and generous wage and pension plans for its employees. The "need" for these services and the city's responsibility for providing them were rarely challenged. Disagreements centered more on the soundness of committing the city's entire increment of new wealth, in anticipation of increased tax revenues, to social welfare services. But things have changed.

City-financed colleges no longer provide tuition-free education. When municipal hospitals are closed, the ensuing protest fails to register on the political Richter scale. Wage freezes are advocated by labor-backed, liberal politicians. Rent control is criticized by all of the city's newspapers, including its major

TABLE 9

New York City Operating Revenues From Intergovernmental Sources: 1961-1977[*]

Fiscal Year	City Revenue (Millions)				Intergovernmental Revenue As A Percent of Total Revenue			Annual Change In Intergovernmental Revenue		
	All Sources	Intergovernmental Sources								
		Total	Federal	State	Total	Federal	State	Total	Federal	State
1961	$2,453.6	$565.2	$110.3	$454.9	23.0%	4.5%	18.5%	—%	—%	—%
1962	2,579.4	628.0	144.3	483.7	24.4	5.6	18.8	11.1	30.8	6.3
1963	2,777.2	729.9	165.9	564.0	26.3	6.0	20.3	16.2	15.0	16.6
1964	3,124.3	781.7	182.6	599.1	25.0	5.8	19.2	7.0	10.0	6.2
1965	3,391.7	873.9	218.3	655.6	25.7	6.4	19.3	11.8	19.5	9.4
1966	3,894.0	1,318.0	307.9	1,010.1	33.2	7.9	25.3	50.8	41.0	54.1
1967	4,616.3	1,738.2	576.8	1,161.4	37.7	12.5	25.2	31.9	87.3	15.0
1968	5,272.4	2,213.2	746.0	1,467.2	41.9	14.1	27.8	27.3	29.3	26.3
1969	6,149.8	2,891.0	899.7	1,991.3	47.0	14.6	32.4	30.6	20.6	35.7
1970	6,590.9	3,052.5	1,059.8	1,992.7	46.3	16.1	30.2	5.6	17.8	0.1
1971	7,824.0	3,647.2	1,286.6	2,360.6	46.6	16.4	30.2	19.4	21.4	18.5
1972	8,650.7	4,041.2	1,524.4	2,516.8	46.7	17.6	29.1	10.8	18.4	6.6
1973	9,444.9	4,596.3	2,048.9	2,547.4	48.6	21.6	27.0	13.7	34.4	1.2
1974	10,138.8	4,843.6	2,049.6	2,794.0	47.8	20.2	27.6	5.4	**	9.7
1975	11,964.7	6,200.1	2,473.7	3,726.4	51.8	20.7	31.1	28.0	20.7	33.4
1976	11,380.8	5,704.0	2,525.1	3,178.9	50.1	22.2	27.9	−8.0	2.1	−14.7
1977	12,497.0	5,445.3	2,357.7	3,087.6	43.5	18.9	24.7	−4.5	6.6	−2.9

[*]Source: *The Role of Intergovernmental Fiscal Relations In New York City*, Temporary Commission on City Finances, Fourteenth Interim Report to the Mayor, May 1977, p. 13.

[**]Less than 0.05 percent.

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black newspaper, the *Amsterdam News*, as being antipoor and a deterrent to economic development. Liberal politicians call for tax cuts and promise a decade of austerity. In short, the virtues of the "welfare city" are no longer considered self-evident. Few votes or accolades are now awarded to those who persist in advocating increased levels of municipal expenditures for social welfare programs.

In a speech delivered on September 10, 1975, one day after the New York State Legislature passed its emergency plan "to hold off default in New York City through a $2.3 billion cash infusion and a state-mandated restructuring of the city's fiscal management,"[25] Mayor Abraham Beame, probably more than he initially realized, charted the new direction for politics in New York City.

> The dilemma of our city really has no villains—only victims. We have made a succession of administrations—local, state and federal—which have responded to public pressures and political needs with new programs and broadened services, most of them well-meaning and most of them fundable in a growth economy. We have seen Mayors, Governors, Presidents and Legislators use all sorts of fiscal gymnastics to find the means to maintain these services. It was especially true in this city—which has a long and compassionate tradition of providing for the poor, the aged and the disadvantaged—who have always looked to this city and the lady standing in the harbor as symbols of hope.[26]

Echoing these comments that Beame made during the early stages of the fiscal crisis, Mayor Edward Koch, at his inaugural, restated the "crisis without a villain—old-fashioned New York liberalism is dead" theme with a fervor and clarity unimaginable a few years ago.

> I do not exaggerate when I say that New York is unique in the history of human kindness. New York is not a problem. New York is a stroke of genius. From its earliest days, this city has been a lifeboat for the homeless, a larder for the hungry, a living library for the intellectually starved, a refuge not only for the oppressed but also for the creative. New York is and has been the most open city in the world and that is its greatness and that is why in large part it faces monumental problems today.
>
> The experiences of the past tells us that money alone is not the answer to our problems. If an ideal city could have been bought with money, we would have purchased and paid for it long ago. If a safe, prosperous city could have been acquired with dollars, we would have it today. But experience has shown that utopia cannot be conjured up with a magic formula of more services, more agencies, more plans and more programs. A better city requires the one ingredient that money cannot buy: people who are willing to give of themselves, people who will fight for a cause they believe in, people who are willing and able to defend and protect the city they love.[27]

[25] Francis X. Clines, "Carey Signs $2.3-Billion Aid Plan Imposing Financial Curbs on City," *New York Times*, September 10, 1975.

[26] "Excerpts from Beame Speech on City's Fiscal Plight," *New York Times*, September 11, 1975.

[27] "Text of Address Delivered by Koch at His Inauguration as Mayor of New York City," *New York Times*, January 2, 1978.

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Wallace Sayre and Herbert Kaufman, in their classic study *Governing New York City*,[28] concluded that there was a viable ("commodious and respectful") pluralism of interests in the city, with islands of bureaucratic control made tolerable by political accommodation. That was true a decade ago. What about today? As the remarks of Beame and Koch make all too clear, the city's social contract is being renegotiated under the whiplash of financial necessity. Its traditions are no longer considered worth fighting for; in fact, many argue that its decline is the result of too much obsequiousness to its outdated traditions. New York City's pluralism was built upon mutual respect and mutual advantage. If the underlying consensus is shattered, if the city's newest arrivals—blacks and Hispanics—think that they are the victims of contemporary racism or past political profiteering so that gaining immediate advantage is their only objective, then accommodation may swiftly turn into anarchy.

How the city's leadership explains as well as allocates its scarce resources will determine whether New York City's great tradition provides a template for another great leap in social thought, or a platform for remorse, recrimination, and rebellion.

[28] Wallace S. Sayre and Herbert Kaufman, *Governing New York City: Politics in The Metropolis* (New York: Russell Sage Foundation, 1960).

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Woodrow Wilson's Political Personality: A Reappraisal

EDWIN A. WEINSTEIN
JAMES WILLIAM ANDERSON
ARTHUR S. LINK

Historians, biographers, and political scientists, as well as psychiatrists and psychologists, have long been intrigued by Woodrow Wilson's personality and its relationship to the events of his academic, political, and diplomatic careers. Although interpretations have varied, there is universal agreement that Wilson's personality was of major importance in both his successes and failures.

Along with the conventional descriptions of Woodrow Wilson's character by his major biographers, Ray Stannard Baker and Arthur S. Link, there have been two book-length psychoanalytic studies. The first, *Thomas Woodrow Wilson*, by Sigmund Freud and William C. Bullitt, was written during the 1930s but not published until 1967.[1] This study, a biased application of a simplistic and distorted version of psychoanalytic theory, is not regarded either by historians or psychoanalysts as a scholarly contribution. In one review,[2] Link demonstrated that the evidence on which the psychological interpretations are based is

[1] Sigmund Freud and William C. Bullitt, *Thomas Woodrow Wilson* (Boston: Houghton Mifflin, 1967).

[2] Arthur S. Link, "The Case for Woodrow Wilson," *Harper's*, April 1967, pp. 85–93.

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wildly inaccurate and at times even fabricated. In another review,[3] the psychoanalyst, Erik H. Erikson, pointed out that the crude psychological propositions used in the book were certainly not the handiwork of Freud; he added, however, that Freud's involvement with the book, no matter how minimal,[4] remains an embarrassment to psychoanalysts.

The second study, *Woodrow Wilson and Colonel House: A Personality Study*, by Alexander L. and Juliette L. George, was published in 1956 and reprinted with a new preface in 1964.[5] In contrast to the Freud-Bullitt book, the Georges' study has received wide acclaim, and political scientists interested in psychology have held it up as a model of a psychodynamically oriented political biography. For example, Bernard Brodie calls the book "the first interpretation of the character of [Wilson] which impressed one as having depth, coherence, and consistency."[6] Fred I. Greenstein and Michael Lerner write that it is "one of the most rigorous and convincing of the psychobiographies of political figures."[7] Elmer E. Cornwell, Jr., in a recent review of books on the presidency, characterized *Woodrow Wilson and Colonel House* as "perhaps the best portrait . . . drawn." "Their analysis of the impact on the future President of a strict Calvinistic creed and demanding Presbyterian-minister father," Cornwell adds, "remains the most successful 'psychohistorical' treatment of any President."[8]

It is our view, however, that the Georges' book (along with an explanatory article that Alexander George published in 1960), although not deserving of immediate dismissal, as is the Freud-Bullitt work, presents an essentially incorrect interpretation of the personality of Woodrow Wilson and its effect on his career.

The style of the book is graceful and concise, and the Georges have an unusual ability to translate psychological concepts into readily grasped, nontechnical language. They are basically sympathetic to Wilson and have great admiration for his achievements. In addition, as Robert C. Tucker has noted in

[3] Erik H. Erikson, in *The New York Review of Books*, February 9, 1967, pp. 3–6; reprinted in *The International Journal of Psychoanalysis* 48 (1967): 462–468.

[4] The degree of Freud's involvement will not be known until the Bullitt Papers at Yale are opened and we can see what Freud actually wrote. It is well known that Freud disliked Americans in general and Wilson in particular, who he thought was the embodiment of the Puritan moralist and was responsible for the breakup of the Austro-Hungarian Empire.

[5] Alexander L. George and Juliette L. George, *Woodrow Wilson and Colonel House: A Personality Study* (New York: Dover Publications, Inc., 1964). Although the book includes an analysis of Wilson's relationship with Colonel House, the central concern is to provide a portrait of Wilson's personality and political behavior (see p. xxi), and it is this portrait, not the analysis of the relationship between Wilson and House, that is the focus of the present essay.

[6] Bernard Brodie, "A Psychoanalytic Interpretation of Woodrow Wilson," in Bruce Mazlish, ed., *Psychoanalysis and History* (New York: Grossett & Dunlap, 1971), pp. 115–123.

[7] Fred I. Greenstein and Michael Lerner, eds. *A Source Book for the Study of Personality and Politics* (Chicago: Markham, 1971), p. 77.

[8] *The Wilson Quarterly* 1 (Winter 1977): 68.

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a recent essay on the book,[9] the Georges deserve credit for advancing, through their case study of Wilson, the understanding that a political leader's personality may play a large or even decisive role in determining his conduct in public affairs. The Georges also made a contribution to the development of the methodology of psychobiography. The two authors, in their Research Note and in the 1964 Preface, and Alexander George in the essay of 1960, described the methods they had used while writing the book and their reasons for adopting these methods; they were the first psychobiographers to attempt to conceptualize the process of applying psychology to biography. Finally, some of the book's shortcomings reflect the time in which it was written. Psychological theory has advanced since the mid-1950s, and a great deal of new evidence about Wilson's life has been discovered since the book's original publication in 1956. The Georges, of course, had to work with the psychological theories and the historical sources which were available at the time.[10]

Nonetheless, the Georges' book, in our opinion, suffers from three major deficiencies, which, taken together, result in an inaccurate portrayal of Wilson's personality. The principal failing of the book is that the research on which it rests is inadequate. Second, the Georges fail to recognize the limitations of their psychological model and misrepresent evidence to fit their theory. Third, they ignore Wilson's neurological disorders as conditions affecting his behavior.

The remainder of this essay will examine in detail two of the topics most crucial to the Georges' interpretation of Wilson's personality—Wilson's childhood and the Princeton graduate college controversy; present data, which though available to them, was not investigated; and comment on their use of psychological theory.

I

The Georges claim that Wilson suffered from a troubled relationship with his father, the Rev. Dr. Joseph Ruggles Wilson, a distinguished southern Presbyterian minister. Although Woodrow Wilson overcame his childhood difficulties to some extent, they state, pathological patterns continued to plague him periodically throughout his political career. The main evidence offered for this view is Wilson's early learning experience. The facts are well-known: Wilson did not learn the alphabet until he was nine and did not read with even minimal facility until he was eleven. He entered school at the age of ten and performed poorly. The explanation offered by the Georges reads:

> Perhaps the Doctor's scorn of fumbling first errors was so painful (or perhaps the mere expectation of such a reaction was so distressing) that the boy renounced the

[9] Robert C. Tucker, "The Georges' Wilson Reexamined: An Essay on Psychobiography," *American Political Science Review* 71 (June 1977): 606–618.

[10] However, Alexander George, in an article published ten years ago ("Power as a Compensatory Value for Political Leaders," *Journal of Social Issues* (July 1968): 29–49), still confidently advanced the theory that Wilson's damaged self-esteem, caused by his tyrannical and demanding father,

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effort altogether. Perhaps, too, failing—refusing—to learn was one way in which the boy dared to express his [unconscious] resentment against his father.[11]

It is improbable that Wilson's slowness in learning to read was due to emotional factors and highly likely that it was a manifestation of developmental dyslexia. Undiagnosed in Wilson's childhood, the condition was well-known to neurologists at the time that the Georges wrote their book. However, a number of psychiatrists and psychologists still believed that the condition was an emotional rather than a neurological problem, and the Georges simply chose an explanation that was currently popular. The condition is a frequent one, occurring in about 10 percent of the school population, significantly more frequently in boys. Research over the past seventy years indicates that developmental dyslexia is caused by a delay in the establishment of the dominance of one cerebral hemisphere—usually the left—for language. One theory is that, with the maturational lag, there is representation of language in both hemispheres and performance suffers in the competition for control of the same function.

The specific evidence that Wilson had developmental dyslexia can be summed up in three groups. First, by his own admission he was a slow reader into adult life. At the Johns Hopkins, the amount of assigned reading was his bane. "Steady reading," he wrote his fiancée, "always demands of me more expenditure of resolution and dogged energy than any other sort of work."[12] Second, difficulties in calculation are commonly associated with developmental dyslexia, and Wilson was extremely poor in arithmetic. Third, when, as a result of a stroke in 1896, he could not write with his right hand, he was able, within a few days and without practice, to write with his left hand in perfectly formed script. This ability strongly suggests that he had mixed cerebral dominance for language.

The reading disability affected Wilson's development and his relationship with his parents, but hardly in the way that the Georges state. It probably made him more dependent on his family. Also, he may have feared that his reading problems indicated that he was stupid or lazy. The condition probably made Dr. Wilson more pedagogic and insistent on drilling his "lazy" son. To judge from Wilson's pleasant memories of having his family read to him, they seem to have been tolerant of his problem. During the Civil War, Dr. Wilson was away from home some of the time serving as a chaplain in the Confederate Army. Much of the reading was done by other family members and, if the Georges' hypothesis were true, Wilson must have been expressing his "unconscious hostility" toward his mother, sisters, and other relatives. We do not have a good

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was an important factor in propelling him into politics. The first, second, and perhaps the third, volumes of *The Papers of Woodrow Wilson*, which shed much new light on the relationship between Wilson and his father, were available when George's article was written. He presumably did not read these volumes; in any event, he did not cite them.

[11] George and George, *Woodrow Wilson and Colonel House*, p. 7.

[12] Wilson to Ellen Louise Axson, March 29, 1884, Arthur S. Link et al., *The Papers of Woodrow Wilson*, 28 vols. to date (Princeton, N. J.: Princeton University Press, 1966–) 3: 496; hereinafter cited as *PWW*.

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record of Wilson's childhood such as might be gained from contemporary letters, diaries, or autobiographies; like any other child, he may have felt insecurities. However, it is certain that they did not come about in the manner postulated by the Georges.

According to the Georges, Wilson's relationship with his father was nothing less than devastating. They say that Wilson's father "ridiculed his intellectual capacities and made him feel mediocre." They add that Wilson had to endure "barbed criticism," "sarcastically made demands" and "aspersions on his moral and intellectual worth," and that Wilson interpreted these criticisms "as humiliating evidence, that, try as he might, he was inadequate." As a result of this treatment, they conclude, Wilson feared that he was "stupid, ugly, worthless and unlovable."[13] The Georges' entire interpretation of Wilson's personality revolves around their portrayal of his relationship with his father. They argue that, because of this relationship, Wilson forever harbored a deep sense of worthlessness and also carried a burden of rage which he later turned against adversaries, such as Dean Andrew F. West and Senator Henry Cabot Lodge of Massachusetts.

The sole evidence for this view of Wilson's relationship with his father, aside from the Georges' interpretation of Wilson's reading disability, consists of comments made to Baker in the mid-1920s. Wilson's daughter, Margaret, talking of Dr. Wilson said: "His idea was that if a lad was of fine tempered steel, the more he was beaten, the better he was."[14] The metaphor, a favorite of both Wilson and his father, may be interpreted in various ways and at several levels of concreteness. Probably Dr. Wilson was boasting about Woodrow. But any interpretation should take into account Margaret Wilson's feelings about her grandfather. Margaret did not know Dr. Wilson well until she was fourteen years old, when, because of his deteriorating health, he came to live with his son in Princeton. Affected by severe cerebral arteriosclerosis, Dr. Wilson had become testy and puerile, and his grandchildren were afraid of him.[15] Margaret Wilson's statement can hardly be regarded as indicative of Wilson's relationship with his father while the boy was growing up.

The other recollection is a more substantial one. Wilson's cousin, Helen Woodrow Bones, told Baker:

> Uncle Joseph was a cruel tease, with a caustic wit and a sharp tongue, and I remember hearing my own family tell indignantly of how Cousin Woodrow suffered under his teasing. He was proud of WW, especially after his son began to show how unusual he was, but only a man as sweet as Cousin Woodrow could have forgotten the severity of the criticism to the value of which he so often paid tribute, in after life.[16]

[13] George and George, *Woodrow Wilson and Colonel House*, pp. 114, 272, 6, 8.

[14] Ibid., p. 8.

[15] Margaret Axson Elliot, *My Aunt Louisa and Woodrow Wilson* (Chapel Hill, N. C.: University of North Carolina Press, 1944), p. 122.

[16] George and George, *Woodrow Wilson and Colonel House*, p. 8.

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This recollection, more than fifty years after the alleged events, may or may not have been accurate. However, Helen Bones (who was the first Mrs. Wilson's private secretary from March 1913 until Mrs. Wilson's death in August 1914, served as mistress of the White House until Wilson married again in December 1915, and continued to live off and on with the president and his second wife) was fiercely and passionately devoted to Wilson. She deeply resented any criticism of the man she idolized.[17] She would probably have exaggerated and magnified the importance of such stories about Dr. Wilson's treatment of his son. She most likely heard the stories from her parents, and there is evidence of bad blood between them and Dr. and Mrs. Wilson.

The Georges cite an additional recollection to give an idea of the nature of this teasing. One morning, according to another cousin, Jessie Bones Brower (Helen Bones's sister), Wilson's father apologized when his son arrived late for breakfast and explained that he had been so greatly excited at the discovery of another hair in his mustache that it had taken him longer to wash and dress. "I remember very distinctly the painful flush that came over the boy's face," the cousin recalled.[18] If this is typical of Dr. Wilson's teasing, it does not sound harsh or excessive and certainly does not suggest the type of father-son relationship that would handicap Wilson for the rest of his life. Wilson's father was a great wit and punster and used his family as an audience, but there is no indication that he was malicious.

Let us grant that these recollections offer some support for the Georges' interpretation. However, they must be weighed against contrary evidence, which is much more substantial. There are many reports from people who knew Wilson and his father well—sources available to the Georges—about the extraordinarily warm relationship that existed between Wilson and his father during Wilson's childhood.[19] They spent a great deal of time together, and Dr. Wilson read to him and talked to him at length. Early in the week, Wilson's father would visit parishioners while they were at work, and he would take his son along to share the experiences. As evidence of Wilson's submissive attitude toward his father, the Georges cite his helping his father, as a young man, with the dull task of writing the minutes of the General Assembly of the southern Presbyterian Church. If one is to judge by the bitter controversy that led to Dr. Wilson's resignation from the Columbia Theological Seminary,[20] the minutes were any-

[17] Expressions of her feelings occur again and again in Helen W. Bones to Jessie Bones Brower, February 12 and 23, April 1 and 12, May 3, and June 1, 7, and 18, 1913, Woodrow Wilson Collection, Princeton University Library.

[18] George and George, *Woodrow Wilson and Colonel House*, p. 8.

[19] See for example, Ray Stannard Baker, *Woodrow Wilson, Life and Letters: Youth, 1856–1890* (Garden City, N.Y.: Doubleday, Page and Co., 1927); Josephus Daniels, *The Life of Woodrow Wilson* (Philadelphia: The John C. Winston Co., 1924); William Bayard Hale, *Woodrow Wilson: The Story of his Life* (Garden City, N.Y.: Doubleday, Page and Co., 1912); William Allen White, *Woodrow Wilson* (Boston and New York: Houghton Mifflin, 1924).

[20] About this episode, see John M. Mulder, *Woodrow Wilson: The Years of Preparation* (Princeton, N.J.: Princeton University Press, 1978), pp. 15–17.

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thing but dull. In pursuit of their theme of Wilson's "unconscious hostility" toward and fear of his father, the Georges go so far as to interpret Wilson's tender care of his father, over the last years of his life, and his singing of the old man's favorite hymns to him, as evidence of Wilson's guilt and submission!

The Georges emphasize that Wilson was extremely dependent on his father until the latter's death. In support of their view, they cite the observation of a Princeton colleague, Winthrop More Daniels, that Wilson was always the pupil of his father. They fail to cite evidence to the contrary, such as that of Bliss Perry, who commented on the comradely relationship between the two men.[21] It is true that Wilson was financially dependent on his father into his late twenties. However, after the death of Wilson's mother in 1888, he, Woodrow Wilson, became the effective head of the family and the one to whom his brother, sisters, and *father* came for advice and support. Yet the Georges maintain that Wilson's early dependence never changed. As further evidence, they cite the famous "my incomparable father" letter written when Wilson was thirty-two and in the first year of his professorship at Wesleyan. The letter reads:

> My precious father,
>
> My thoughts are full of you and dear "Dode" [Wilson's younger brother, Joseph] all the time. Tennessee seems so far away for a chap as hungry as I am for a sight of the two men whom I love. As the Christmas recess approaches I realize, as I have so often before, the *pain* there is in a season of holiday and rejoicing away from you. As you know, one of the chief things about which I feel most warranted in rejoicing is that I am your son. I realize the benefit of being your son more and more as my talents and experience grow: I recognize the strength growing in me as of the nature of your strength: I become more and more conscious of the hereditary wealth I possess, the capital of principle, of literary force and skill, of capacity for first-hand thought; and I feel daily more and more bent toward creating in my own children that combined respect and tender devotion for their father that you gave your children for you. Oh, how happy I should be, if I could make them think of me as I think of you! You have given me a love that grows, that is stronger in me now that I am a man than it was when I was a boy, and which will be stronger in me when I am an old man than it is now—a love, in brief, that is rooted and grounded in *reason*, and not in filial instinct merely—a love resting upon abiding foundations of *service*, recognizing you as in a certain very real sense the author of all I have to be grateful for! I bless God for my noble, strong, and saintly mother and for my incomparable father. Ask "Dode" if he does not subscribe? and tell him that I love my brother passionately.
>
> . . . Ellie joins me in unbounded love to you both.
>
> Your devoted son,
> Woodrow[22]

Although still an important figure in the southern Presbyterian Church, Dr. Wilson had encountered disappointments in his work and was frequently discouraged and depressed. His career had not advanced, while Woodrow's had, and he had become increasingly identified with and somewhat envious of his

[21] Bliss Perry, *And Gladly Teach* (Boston: Houghton Mifflin, 1935), p. 153.
[22] Wilson to Joseph Ruggles Wilson, December 16, 1888, *PWW*, 6: 30–31.

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son. Wilson, busy in his new position at Wesleyan and involved with writing *The State* (1889), had not written to his father for some time. Could not Wilson, feeling his neglect of his father, now a lonely widower, have written the letter to raise the old man's spirits and self-esteem? And could he not in fact have been expressing his feelings toward his father?

The Georges claim that Dr. Wilson became proud of his son only after the latter began to show how unusual he was. There could be some argument about when Woodrow Wilson began to display extraordinary talent. Probably it was during his senior year at Princeton, when he was managing editor of *The Princetonian* and had an article accepted by a national review. Yet the many letters that Joseph R. Wilson wrote to his son before 1878–79 reveal affection, admiration, pride in his son, and helpful criticism, but never denigration.[23] Wilson dedicated *Congressional Government* to his father. He wrote:

> To his father, the patient guide of his youth, the gracious companion of his manhood, his best instructor and most lenient critic, this book is affectionately dedicated by the author.

Dr. Wilson had a tremendous influence on his son, and probably no boy grows up without some resentment toward his father. However, on the basis of the evidence there is no reason to believe that the dedication of *Congressional Government*[24] was not a valid representation of Wilson's feelings toward his father.

It is almost incredible that a psychobiography tracing the development of Wilson's personality should make so little mention of his mother, Janet, or Jessie, Woodrow Wilson, about whom a great deal was known when the Georges wrote their book. They spend less than half a page on the subject, quoting from the memories of her that Wilson expressed at the time of her

[23] None of these letters was written during Wilson's early childhood; they date from the year 1875, when Wilson was eighteen years old, and they include seventy-six letters written before September 1885, when Wilson began his first teaching job at Bryn Mawr College. They are printed in *PWW*, vols. 1-5.

[24] *Congressional Government: A Study in American Politics* (Boston: Houghton Mifflin, 1885), p. iii.

Thirty years later, long after his father's death, we hear Wilson saying about his father: "I remember my father as essentially humble and devout before God, spirited and confident before men. A natural leader, with a singular gift of clear, eloquent, convincing speech. Generous, playful, full of high spirits, and yet given to most laborious industry, alike in the use of books and in the administration of church affairs. A man who believed much more in the efficacy of Christian simple and pure living than in dogmatic advice or spiritual conversation,—a robust Christian.

"The description I naturally associate with his name would be something like this:

"Generous, high-spirited, thoughtful, studious, a master of men and of thought, who sought the life of dogma in conduct and preferred a pure life to a pious profession" [c. May 25, 1914].

And again: "As I look back on my boyhood, it seems to me that all the sense I got, I got by association with my father. He was good fun; he was a good comrade; and the experience he had had put a lot of sense in him that I had not been endowed with by birth, at any rate; and by constant association with him, I saw the world and the tasks of the world through his eyes, and because I believed in him I aspired to do and be the thing that he believed in" [May 29, 1914].

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death: "As the first shock and the acute pain of the great, the irreparable blow passes off, my heart is filling up with tenderest memories of my sweet mother, memories that seem to hallow my whole life—which seem to explain to me how it came about that I was given the sweetest, most satisfying of wives for my daily companion. My mother, with her sweet womanliness, her purity, her intelligence, prepared me for my wife. I remember how I clung to her (a laughed-at 'mamma's boy') till I was a great big fellow: but love of the best womanhood came to me and entered my heart through her apron-strings."[25]

A discerning reading of all the evidence about Jessie available to the Georges and of the Wilson family correspondence, which was not available to them, would have shown that this description of Wilson's relationship with his mother was far from complete. Jessie Wilson was indeed a saintly, intelligent woman, devoted to her family, and Wilson was a very loving son. But she was extremely overprotective, and the mutual dependence that she fostered was responsible for the relatively late age at which Wilson achieved emotional and heterosexual maturity. Shy, reserved, and deferential to the outgoing Dr. Wilson, she was intensely ambitious for her husband and son. She was also extraordinarily sensitive to what she felt were slights of others; and, when Wilson or his father encountered setbacks, she invariably attributed them to the spite and malice of their opponents. While Wilson's father pointed out his son's faults and tried to temper his impatience and aggressiveness, she was completely uncritical. Much more than Dr. Wilson, she gave Woodrow the feeling that he was not only intellectually, but morally, superior to his colleagues—a trait that contributed to one of Wilson's greatest failings, his overconfidence. Her influence had a great deal to do with Wilson's shyness and uneasiness with people who were not close to him, and his particular dependence on women. Mrs. Wilson was often ill—probably on a psychosomatic basis—and extremely anxious and worrisome about Woodrow's health. Wilson's attitudes toward illness, derived from both parents, had most important consequences for his career. There is also good evidence that Mrs. Wilson suffered from chronic depression; her son later said many times that he had inherited his tendency toward melancholy from his mother's side.

II

No study of Wilson's behavior during the great crises of his career can be complete without taking into account the effects of the cerebral vascular disease from which he suffered, intermittently, from the age of thirty-nine until his death in 1924. In 1896, Wilson had a stroke which caused a marked weakness of his right hand, and he had recurrences in 1900, 1904, 1906, and 1907. The stroke of 1906 caused not only weakness of his right arm but almost complete blindness of his left eye, a syndrome indicative of an embolus from the left internal carotid artery. Wilson's attempts to adapt to the consequences

[25] WW to Ellen A. Wilson, April 18, 1888, *PWW*, 5: 719.

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of the stroke caused changes in his behavior and helped set into motion events relevant to his defeats in the quadrangle plan and graduate college controversies at Princeton. The course of carotid artery disease is very variable, with frequent remissions, and, in the light of today's knowledge, it is not surprising that Wilson was in good health while governor of New Jersey and when he became president of the United States. The disease, however, is ultimately a progressive one and, by the end of World War I, Wilson was showing signs of generalized cerebral involvement. In Paris, he had an attack of influenza, probably a virus encephalopathy, which, superimposed on pre-existing brain damage, produced changes in behavior which may have influenced the outcome of the peace negotiations. In October 1919 he had a massive stroke that completely paralyzed the left side of his body and produced mental attitudes and personality changes which were important factors in his failure to obtain ratification of the Treaty of Versailles.[26]

The Georges are not neurologists or psychiatrists, and some of the information on which the above reconstruction is based was not available to them. Yet even lay observers, such as Herbert Hoover, had commented on the changes of Wilson's mental functioning after the Paris illness. Any behavioral scientist should have wondered about the effects of the massive stroke of 1919. Similarly, the Georges ignored Wilson's stroke of 1906 as a factor in his behavior and explained his problems during the quadrangle (or "quad") plan and graduate college controversies as stemming from an emotionally disturbed childhood.

The controversy over the location and character of the graduate college was the most dramatic event of Woodrow Wilson's term as president of Princeton University. A wealthy alumnus, William C. Procter, in 1909 offered to donate $500,000 to help build a graduate college to house graduate students, but he included an important stipulation—that the building be located on the golf course, nearly a mile from the center of the campus. It was the site which the dean of the small existing graduate school, Andrew Fleming West, a close friend of Procter, had selected. West favored an off-campus site so that graduate students might study undisturbed by undergraduates. Wilson consistently argued that they should live close together, in order, as he put it, that the graduate students might energize the intellectual life of the undergraduates.[27]

[26] See Edwin A. Weinstein, "Woodrow Wilson's Neurological Illness," *Journal of American History* 57 (September 1970): 324–351. On a basis of new evidence, it is not likely, as was stated in this article, that Wilson had a stroke in Paris.

[27] As Wilson put it in an address before the Princeton Club of New York on April 7, 1910: "As I look back upon my own studies as a graduate student, I realize that . . . the intellectual limitations of the men with whom I was associated in the great graduate school I attended, was that their thought was so centered upon special lines of study that they had become impatient of everything that drew them away from them. I have heard these men again and again deplore the necessity they would some day be under of going through the drudgery of teaching stupid undergraduates,—a state of mind and a point of view which utterly unfitted them for the very profession they were approaching. That was due, in my mind, to the dissociation of graduate study in the life of the university from the undergraduate body.

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It was the same idea that had led Wilson to institute the preceptorial system and to propose his quadrangle plan. Originally, both West and Wilson had advocated a central location, but West had changed his view. West believed that Wilson had delayed action on the graduate college in favor of his preceptorial and quad plans—which indeed he had—and charged that Wilson had gone back on his promise of support, given in 1906, at the time when West turned down an offer of the presidency of the Massachusetts Institute of Technology. Wilson believed, with ample reason, that West was an intellectual dilettante, who wished to maintain dominion over an elitist world of his own. Some months before the Procter offer, Wilson had reorganized the faculty committee on the graduate school and broken West's absolute control of the policies of the graduate school.

The Georges see the conflict as based on Wilson's perception of West as a father figure. In their view, the situation caused Wilson to give vent to the unconscious hostility which he had felt, but had never been able to release, toward his own father. They regard the issue of the location of the college as merely an excuse for Wilson to release this unconscious hostility by doing battle with West. At some level, they say, West evoked in Wilson the image of his father. Therefore Wilson resisted West "with all the violence he had once felt, but never ventured to express, in response to his father's overwhelming domination." But, they add, Wilson could not confess, even to himself, that he experienced West as "an unbearable threat," and, consequently, he "disguised all his volcanic feelings against West in terms of a great moral crusade in which he championed the 'right.' "[28]

We have already examined the Georges' previous evidence for Wilson's "unconscious hostility" toward his father. Before considering further their interpretation of the graduate college controversy, additional background is necessary. The situation was already a tense one because of the bitter struggle that had occurred over the quadrangle plan. This was a proposal by Wilson to abolish the exclusive undergraduate eating clubs and construct quadrangles, or residential colleges, assignment to which would be made by lot. Opposition, often violent, soon developed and was led by the eastern alumni. The trustees, who had approved the plan "in principle," when Wilson presented it in June 1907, withdrew their support in October. Wilson continued to press for it, and the battle became so acrimonious that a wealthy trustee and generous contributor to the university, M. Taylor Pyne, threatened to withdraw his support if Wilson ever mentioned the quad plan again.[29] Dean West led the minority

"Not only that, but I believe that our universities lack the impulse of advanced study, so far as their undergraduates are concerned, largely because their undergraduates are not sufficiently brought into contact with the graduates. You know, gentlemen, that the process of education is a process of contagion. If you want to educate a man, put him in close association with the kind of intellectual fire you wish him to be touched by" (*PWW*, 20: 345).

[28] George and George, *Woodrow Wilson and Colonel House*, pp. 40, 43.

[29] Melancthon W. Jacobus to Wilson, November 5, 1907, *PWW*, 17: 468–69.

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faction of the faculty who were in opposition, which, to Wilson's great sorrow, included his best friend, John Grier Hibben.

Wilson's illness was an important factor in the defeat of his quad plan and also laid the groundwork for his defeat in the graduate college controversy. In 1906 he was at the height of his popularity and power at Princeton: the curriculum had been reformed, the faculty upgraded, and the preceptorial system successfully launched. Except for an episode of weakness in his right arm lasting several months in 1904, he had been in excellent health during his presidency. The stroke of 1906 was the first one to be recognized as such, and Wilson received a gloomy prognosis. He and his wife were aware that the condition was similar to that which had robbed his father of his mental faculties. Characteristically, Wilson's reaction was to immerse himself in his work. Originally, he had thought that the quad plan would take twenty years; now he thought it immediately attainable. In the interests of his health, he cut down on his social life. He took regular naps, reduced his personal contacts, and, in contrast to his previous habits, now decided to meet members of the Princeton community only on official business. Prior to the formal announcement to the trustees of the plan in June 1907, he had mentioned the idea to only three intimate friends on the faculty and not at all to the alumni. This lack of preparation contrasted markedly with the way he had organized support for the preceptorial system and must be judged an important cause of his failure. Wilson's increased overconfidence was one of a number of behavioral changes that occurred after the stroke in 1906; he became less empathic, more stubborn, and more prone to overgeneralize and personalize his problems.

The question of the site of the graduate college was far more than a pretext for Wilson to destroy West. At stake were Wilson's control of the university and the character of its educational program. It is debatable whether close association between graduates and undergraduates would have yielded the advantage that Wilson envisaged. However, his emphasis on such an association in the preceptorial and quadrangle plans indicates that the issue was important in itself. The Georges' treatment of the graduate college controversy makes it seem that Wilson stood practically alone in pressing his arguments. Actually, many others shared his point of view. Approximately half of the trustees supported Wilson, and the various votes related to the controversy were all close. Although the evidence is not conclusive, it appears that a large majority of the faculty, particularly the professors who were doing most of the graduate instruction, preferred that the college be built in the midst of the campus. In fact, members of the faculty (and of the board of trustees) were constantly pressing Wilson to stand firm and even to remove West or reduce his influence over the graduate program and college.[30]

[30] Arthur S. Link wishes to state that much new evidence discovered since the publication of his *Wilson: The Road to the White House* in 1947 has convinced him that his treatment of Wilson and West and the graduate college controversy is inadequate and unbalanced.

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Wilson's personality and his pattern of behavior under stress had a great deal to do with the events of the controversies over the quad plan and graduate college. But Wilson's aversion to Dean West did not come about because of unconscious feelings toward his father. Wilson detested West because West was the condensed symbol of so many of the social, educational, economic, and political problems that Wilson faced at Princeton. Moreover, Wilson did not "disguise" his feelings toward West. He expressed them quite openly.[31]

It is impossible in a single article to provide detailed analysis of and commentary on an entire book. It must suffice to say that we believe that the Georges' view of the relationship between Wilson and Edward M. House was highly distorted by overreliance on the House diary, which often presents a one-sided and unreliable account of events; that we find their explanation for Wilson's attempts to remain out of World War I almost arcane; and that recent research has considerably revised their explanation for the so-called break between Wilson and House at the Paris Peace Conference.[32]

III

By today's standard, the Georges' use of psychological theory is inadequate. Their formulations have been largely superseded by the views of ego psychology and by considerations of the development of the self and of social roles and interactions. While the Georges cannot be criticized for not anticipating these approaches, it should be pointed out that the works of Harry Stack Sullivan, Karen Horney, and Erich Fromm were available to them. Also, the commonsense view that most children identify with their parents and grow to resemble them does not seem to have played a part in their thinking. By the 1950s, the idea of fixed childhood hostilities, locked deep in the unconscious, impervious to change, and ready to burst forth in pristine fury, was being challenged. For the Georges to suggest that Wilson simply and directly transferred his hatred of his father to West and Senator Lodge during the fight over the Versailles Treaty is highly mechanistic, reductionistic, and deterministic. The formulation leaves no room for consideration of character development or identity formation.

The authors could have given a more realistic view of Wilson's relationship with his parents had they taken up family values and cultural norms. For example, Wilson's reluctance to express any negative feelings toward members of his family was shared by them and was a facet of their subculture. It is difficult to accept any theory of personality that does not take into consideration

[31] The entire graduate college controversy is amply documented in *PWW*, vols. 18–20.

[32] The most authoritative study of House at the Paris Peace Conference, Inga Floto, *Colonel House in Paris, A Study of American Policy at the Paris Peace Conference 1919* (Aarhus, Denmark: Universitetsforlaget, 1973), proves conclusively that the cause of the so-called break between the two men was House's failure to follow Wilson's explicit instructions while the latter was in the United States.

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a subject's relationship with his mother, particularly Wilson's mother, who instilled so much anxiety in him. Then, the Georges give the impression that all that one has to know about Wilson is that his self-esteem was disastrously low. Yet self-esteem is rarely unitary. One may feel confident in some areas, insecure in others. Wilson was bothered by feelings that he was ugly; he seems to have identified himself in this respect with his rather physically unattractive mother. Yet he rarely had doubts about his intellectual and leadership qualities after reaching maturity.

Even without detailed knowledge of Wilson's childhood, one would assume, from his compulsive personality traits and his need to control his environment, that he had anxieties and insecurities. Yet to translate such insecurities into an insatiable drive for political power is to provide far too simple an explanation for a complex, multidetermined process. Further, the Georges state:

> His [Wilson's] stern Calvinistic conscience forbade an unabashed pursuit of power for personal gratification. He could express his desire for power only insofar as he convincingly rationalized it in terms of altruistic service and fused it with laudable social objectives.[33]

It is true that Wilson often validated pragmatically arrived at decisions in moral terms, but when in the world of human affairs is concern for the public good divorced from personal gratification?

Woodrow Wilson and Colonel House has important implications for writers of psychobiography. They should be aware that personality theories derive, not only from scientific investigations, but also from ideology, the social milieu, and the personal problems of their authors. Theories are dated, as the Georges themselves point out; theories not only change with the years, but proponents of the same theory differ in their interpretations of it. In an interdisciplinary endeavor, more than superficial knowledge of areas outside of one's primary field is necessary. The historian or political scientist should be familiar, not only with psychoanalytic theory, but also with the many aspects of behavioral science, which, in recent years, have so profoundly influenced psychoanalysis. Above all, the psychobiographer should immerse himself in the primary biographical sources, not least of all for the reason that the criteria for clinical evidence may be quite different from those for historical work.[34] The great acclaim accorded the Georges' book may have helped give the impression that psychobiography can be written by furnishing "deep" interpretations of the work of others. No psychobiography is better than the research on which it rests.

[33] George and George, *Woodrow Wilson and Colonel House*, p. 117.

[34] The best example in the past few years of a psychiatrist's use of primary historical sources is John E. Mack, *A Prince of Our Disorder: The Life of T. E. Lawrence* (Boston: Little, Brown, 1976).

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Control and Responsibility in the Powerful Professions

BERNARD BARBER

Everywhere in the United States the professions have reached new heights of social power and prestige. Everywhere, because of the power of their special knowledge, they are of increasing consequence in the lives of individuals and in the affairs of groups, the polity, and the society as a whole. Yet everywhere they are also in trouble, criticized for their selfishness, their public irresponsiblity, their lack of effective self-control, and for their resistance to requests for more lay participation in the vital decisions professionals make affecting laymen.

The signs of this trouble are manifest in many quarters. In California the governor has for the first time appointed laymen to every one of the state boards that regulate professional conduct; formerly these boards were monopolized and dominated, run in their own interests, by the professionals themselves. In New York State the legislature has passed new laws requiring more effective peer control over delinquent medical practitioners.[1] Also in New York, The Board of Regents, which has responsibility for the public control of some twenty-nine "professions," has recently voted to permit professions to do limited advertising in newspapers and magazines. Further, it has issued a new regulation requiring health professionals to show a patient his medical records upon request, except when this "would adversely affect the patient's health."[2] This latter exception is an application of the therapeutic privilege, which sets certain limits on the

[1] *New York Times*, April 29, 1977.
[2] *New York Times*, July 22, 1977.

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patient's right to know, the doctor's duty to tell. In 1973 the U.S. Congress established the National Commission for the Protection of the Subjects of Biomedical and Behavioral Research to reduce long-standing abuses against the persons and civil rights of the subjects of biomedical and social research. In 1974 a report on medical malpractice was issued by a commission appointed by the secretary of the Department of Health, Education, and Welfare.[3] As the problem of medical malpractice continued to become more severe, the State of New York appointed its own investigatory panel which issued its report in 1976.[4] In the same year, another Senate committee, the Committee on Government Operations, investigated the malfeasance of another powerful profession, the accounting profession.[5] The "arrogance" and "elitism" of the academic profession, which trains and sets standards of self-control and responsibility for the other professions, have also recently been criticized.[6] So widespread and recurrent is public protest against the irresponsible power of the professions that two sociologists have referred to "the revolt of the client" as a now endemic phenomenon in American society.[7]

Obviously there now exists what sociologists call a "social problem," that is, a set of social conditions defined by at least some articulate groups in the population as not only morally bad but urgently in need of reform. The excessive power, the lack of satisfactory self-regulation, and the public irresponsibility of the professions is one of our new "social problems."

Just as obviously, at least to sociologists, there is a sociological problem here, or better, a set of them. If public policy is to be effective in achieving some reform of the present defects of the professions, it needs satisfactory analyses of several sociological problems that underlie these defects. What is "a profession" and how are control and responsibility essential characteristics of professions? What evidence or measures can we produce to describe the alleged defects of control and responsibility in the professions? And what reforms of the mechanisms

[3] U.S. Department of Health, Education, and Welfare, *Report of the Secretary's Commission on Medical Malpractice* (Washington, D.C.: U.S. Government Printing Office, 1973).

[4] State of New York, *Report of the Special Advisory Panel on Medical Malpractice.* Albany, N.Y., 1976.

[5] U.S., Senate, Committee on Government Operations, Subcommittee on Reports, Accounting and Management, *The Accounting Establishment* (Washington, D.C.: U.S. Government Printing Office, 1976).

[6] See Dorothy Nelkin, *Science Textbook Controversies and the Politics of Equal Time* (Cambridge, Mass.: MIT Press, 1977); Dorothy Nelkin, "Scientists and Professional Responsibility: The Experience of American Ecologists," *Social Studies of Science* 7(1977): 75–95; Joseph Ben-David, "Science as a Profession and Scientific Professionalism," in J.J. Loubser et al., eds., *Explorations in General Theory: Essays in Honor of Talcott Parsons* (New York: Free Press, 1976); Allan Mazur, "Disputes between Experts," *Minerva* XI(1973): 243–262; and Allan Mazur, "Opposition to Technological Innovations," *Minerva* XIII(1975): 58–81.

[7] M.R. Haug and M.B. Sussman, "Professional Autonomy and the Revolt of the Client," *Social Problems* 17(1969): 153–161; M.R. Haug, "The Deprofessionalization of Everyone?" *Sociological Focus* 8(1975): 197–213; and M.R. Haug, "The Erosion of Professional Autonomy: A Cross-cultural Inquiry in the Case of the Physician," *MMFQ/Health and Society* (Winter 1976): 83–106.

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of social control and public responsibility of the professions are now occurring or can be suggested on the basis of sociological analysis and data?

A Definition of Professional Behavior

Professional behavior is defined in terms of three essential and somewhat independent variables: powerful knowledge, self-control or autonomy, and public responsibility or direct service to the public and the public welfare.[8] An occupation is the more professional the more it actually displays, not just claims to possess, of these three characteristics. What holds as between occupations, also holds within occupations. Not only are clergymen more professional by our sociological definition than morticians, but some clergymen, by the same standards, are more professional than other clergymen. There is always some range of professionalism within any large occupational category which is publicly defined as professional. But this range may vary in different categories of professionals. For example, the range between the most and least professional physician is less than between the most and least professional clergyman. The matter of degree of professionalism, whether between or within occupational categories is always important; the sociology of the professions needs to create better measures of these differentials than we now have. Such measures are needed for each of the three essential defining variables. A professional category may have more of knowledge and less of effective self-control and actual public responsibility. And its degree of knowledge, or control, or responsibility may be changing over time, to more or less. Such changes are matters of sociological and practical consequence. Now let us consider each of the three defining variables more closely.

POWERFUL KNOWLEDGE

There are two generalized bases of power or consequentiality in systems of human action.[9] One is knowledge, the other is the capacity for making decisions

[8] On the theory of the professions, see E. Durkheim, *Professional Ethics and Civic Morals*, trans. C. Brookfield (London: Routledge & Kegan Paul, 1957); T. Parsons, "The Professions and Social Structure," *Social Forces* 17(1939): 457–467; Bernard Barber, "Is American Business Becoming Professionalized? Analysis of a Social Ideology," in E.A. Tiryakian, ed., *Sociocultural Theory, Values, and Sociocultural Change: Essays in Honor of P.A. Sorokin* (Glencoe, Ill.: Free Press, 1963); Bernard Barber, "Some Problems in the Sociology of the Professions," *Daedalus* (Fall 1963): 669–688; Bernard Barber, "Compassion in Medicine: Toward New Definitions and New Institutions," *New England Journal of Medicine* 295(1976): 939–943; W.E. Moore, *The Professions: Roles and Rules* (New York: Russell Sage Foundation, 1970); Nathan Reingold, "Definitions and Speculations: The Professionalization of Science in America in the Nineteenth Century," in A.C. Oleson and S. Brown, eds., *Knowledge in the Early American Republic* (Baltimore, Md.: Johns Hopkins University Press, 1976); and Harold Wilensky, "The Professionalization of Everyone?" *American Journal of Sociology* 70(1964): 137–158.

[9] Bernard Barber, *Social Stratification* (New York: Harcourt Brace, 1957), and Bernard Barber, "The Limits of Equality: Social Stratification in Complex Societies," *Sociological Inquiry*, forthcoming.

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in the informal and formal organizations into which human action gets structured. By knowledge we mean to refer to the whole range of symbols or idea-systems which define the means and ends, the interests and values, the beauties and the ultimate meanings of human action. Though some occupations combine a mix of knowledge and decision making, most tend to be characterized chiefly by one or another of these two generalized dimensions of power. The professions are those occupations that specialize in the development and application of powerful knowledge. Lawyers, for instance, specialize in the knowledge that affects our central interests, our deepest values, our very sense of equity and justice. Physicians control the knowledge that helps us achieve our interrelated senses of physical, psychological, and moral well-being. The theology and preachings of clergymen define our approach to good and evil in human affairs, our aspirations for meaningfulness, our ability to cope with human finitude and death. Academic scientists—physical, biological, and social—also define our frames of meaning by their theories of the nature of the world and its evolution, their understanding of the essence of life and its processes, and by their ideas about power and justice in social life. Artists of various kinds mold our sense and appreciation of beauty and meaning in the world.

Knowledge is always power, and each of the professions has its own special powerful knowledge. The more powerful professions, or parts of professions, are those whose knowledge is generalized and systematic yet with some application to the empirical physical, biological, or social worlds. The irony of power in knowledge is that the more it is based on generalized and systematic theory, the more, soon or late, it has important consequences for systems of human action. Common sense, with its tendency to stress the immediacy and specificity of power, rejects this paradox in the power of knowledge, but sociological analysis and research confirm it. The "semi-professions,"[10] or the semi-professional parts of those professions where there is a range of control over generalized and systematic knowledge, are those with the more concrete, less organized knowledge.

EFFECTIVE SELF-CONTROL OR AUTONOMY

Because generalized and systematic knowledge is esoteric, known to and controlled by only a relatively few occupational specialists as a result of considerable and continuing training and work, its development and application require a considerable amount of self-control or autonomy for those who specialize in these tasks. This self-control is effected through a number of different standard social control mechanisms, such as the inculcation of high cognitive and moral standards, the use of informal peer controls, formal organizational mechanisms within the profession, and, finally, the domination by the profession of a variety of external legal and political mechanisms in the area of licensing, standard-setting, and sanctions for deviant behavior. Autonomy and self-regulation in

[10] Amitai Etzioni, *The Semi-Professions and Their Organization* (New York: Free Press, 1969).

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these various forms are not something illegitimately seized by the professions but approved by public opinion and granted by formal action of various political bodies. (Remember, we are presenting a definition here, not describing any actual social reality. That is a matter always requiring investigation.)

PUBLIC RESPONSIBILITY OR DIRECT SERVICE TO THE PUBLIC WELFARE

Just because of its consequentiality for human affairs, power of any kind is always restricted in some measure by social and political controls. The power of every kind of specialist is always too important to the public welfare for it to be left to the specialists themselves. In the case of professional power, the necessary granting of a considerable degree of autonomy to the professions imposes on them an equally considerable degree of obligation to exercise their power clearly and directly for the welfare of their clients and the public. Professionals acknowledge and are even proud of this special obligation. They value their special role as "fiduciaries" and "public servants." In the name of this special obligation to display what Talcott Parsons has called "collectivity-orientation," the professions ask for public "trust" and compliance with their "orders." A good deal of this trust is willingly given, but nonetheless, even for the professions, autonomy is never granted as an absolute. Both the public and its political representatives always exercise some controls to ensure the actual public responsibility and service behavior of the professions.

Defects in Control and Responsibility in the Powerful Professions

Our analytical definition states the essential theoretical nature of the professions and the conditions under which professions operate for the best interests and values both of professionals themselves and of the publics they serve. Our definition provides *a measure* for actual empirical social reality, *a guide* for investigating the degree to which actual professions do or do not fulfill the essential criteria of professionalism.

It is probably the case that these essential defining characteristics of a profession have seldom been anywhere near fully realized in the history of actual professions. Berlant, for example, examining the history of codes of medical ethics, has argued that these codes have, from Hippocratic times right down to the present, contributed more to a monopoly of power and interest for the profession than was good for the public welfare.[11] Certainly no actual profession comes up to these standards at present. Certainly also, as we have seen, a variety of voices is now expressing its dissatisfactions with present defects in control and responsibility patterns in all the powerful professions. These several

[11] J.L. Berlant, *Profession and Monopoly* (Berkeley: University of California Press, 1975).

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complaints about the different professions are often the same for all of them, but there are also some special defects attributed to each. We shall look separately at alleged defects in four professions.

Before doing so, we may ask why it is that complaints about control and responsibility defects in the professions are so numerous and strong just now. There are at least two structural sources of the present complaints. The first lies in the utilitarian interests of the public. The professions are more powerful now than they have ever been and, consequently, they have more power to hurt the public when their control and responsibility mechanisms are not working satisfactorily. The second source is in the changing values of the public, or rather, in the increasingly strong emphasis the public is putting on the old value of equality. The public has less tolerance for professional domination or even paternalistic benevolence. It wishes to participate more fully and more equally in making the decisions that affect its vital interests, both at the level of individual encounter with professionals and at the level of national political decision. In medical therapy and research, for example, patients and subjects wish to participate only on the basis of voluntary and informed consent; with regard to the terms of payment for medical care, they will no longer allow the American Medical Association to dictate what those terms should be. The defects of the powerful professions may be no greater today than in the past, but a new combination of utilitarian and value concerns in the public demands that these defects be reduced or eliminated.

THE MEDICAL PROFESSION

In the public mind, physicians probably constitute the prototype and premier profession. Their knowledge, their prestige, and their power are all considered to be larger than those of other professions. Nonetheless, there are widespread complaints against the medical profession on the grounds both of failures in the realm of service to the public and of defects with regard to effective self-regulation. So far as unsatisfactory service is concerned, the public dislikes the way physicians often seem to be concerned more for science than for caring, to have turned their means into ends, to have become authoritarian and unresponsive, and, finally, to care too much about their money income.

With regard to these several defects, of course, individual physicians differ considerably. But the social control mechanisms by which the best and the average might make the worst come up to higher standards are not very effective. Informal peer controls, as Freidson's research and analysis have shown, do not work well.[12] Physicians who, in group practices, in hospitals, or otherwise, see the faults of their colleagues usually take no action to correct them. They may avoid or even ostracize such colleagues, but these colleagues are

[12] Eliot Freidson, *Professional Dominance* (New York: Atherton Press, 1970); Eliot Freidson, *Profession of Medicine* (New York: Dodd, Mead, 1970); and Eliot Freidson, *Doctoring Together* (New York: Elsevier, 1975).

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then free to commit their harms elsewhere. Those few physicians, nurses, or other paramedicals who go against the system and report incompetent physicians may find themselves silenced or punished. Recently there has been some change of attitude within the profession toward this ineffective informal system of peer control. In New York State, leaders of medical societies are now supporting proposed legislation that would require doctors who learn of acts of professional misconduct to report them to the State Board for Professional Conduct.[13] Under this legislation, failure of a doctor to report "untoward incidents" would itself be professional misconduct that could jeopardize the silent doctor's license. To protect a reporting doctor from legal reprisals, the legislation grants him immunity from civil suits. The sponsors of this legislation are said to feel that it "will lift the veil of silence in the medical profession which protects doctors who are incompetent, ill, or venal and who endanger the health and lives of their patients." Not all physicians are happy about this proposed legislation. In a letter to the editor, one physician calls it "draconian" and a "police-state solution."[14] Since formal rules do not guarantee informal practices, there is no assurance that the legislation will achieve its goal of more effective self-regulation among physicians. Still, it is a sign of a defect that needs a remedy.

Formal control mechanisms established by organizations of physicians have not been much more effective than informal peer control. In each of the local county medical societies into which the American Medical Association is divided for organizational purposes, there exist ethical conduct committees. But, until quite recently, these committees have been minimally effective or even entirely inoperative. And at a still higher level, at the level of what are really the ultimate formal control mechanisms, the state medical licensing boards, which are dominated by the professionals themselves, social control has been weak.[15] It is only with the greatest difficulty that his license can be taken away, even from a physician who is an alcoholic or a narcotic addict.

While there are several sources of the current "malpractice crisis," or the rise in the number of malpractice suits, one of them is undoubtedly just these defects in the informal and formal control mechanisms among physicians. When an aggrieved patient can get no comfort or aid from other physicians, from the ethics committee of the local medical society, or anywhere else in the medical realm, he or she is the more likely to go to the legal system as a last resort. This source of malpractice suits was recognized by the New York State Special Advisory Panel on Medical Malpractice (1976). To keep the risk of medical injury to a minimum, it said, "will require strengthening quality controls over medical practice through a variety of measures, including disciplinary measures for substandard practice, vigorous controls over hospital staff privileges, and

[13] *New York Times*, April 29, 1977.

[14] *New York Times*, June 11, 1977.

[15] H. Lewis and M. Lewis, *The Medical Offenders* (New York: Simon and Schuster, 1970), Part I, "The Crisis in Medical Discipline."

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limitations of physician practice to areas of proven competence as demonstrated by continuing education and relicensure, if needed."

Medical therapy is not the only part of professional practice that is now being criticized as defective. Medical research using human subjects has also been so criticized, not only in the infamous case of the Nazi doctors, but in the standard procedures of American medical research.[16] Good research evidence shows that at least a significant minority of research physicians has been doing research where risk to subjects exceeded benefits, where satisfactory informed consent was not being obtained, and where the poor and uneducated were more likely to suffer undue risk and be ill-informed than the well-to-do and well-educated. One socially structured source of these defects is the heavier weighting which research physicians give to science as against humane therapy. Another is the lack of education in the ethics of research in the medical schools, in residency training, and in actual clinical practice. There has been some small improvement in this regard, recently, in the form of numerous courses and lectures at the medical schools, but there is still no good evidence that the "scientific stars" who dominate many of these medical schools are treating the ethics of research with the same seriousness with which they take their scientific research itself.[17] The data show that the leading medical centers have not been leaders in concern with the ethics of research as they have been in scientific excellence.[18] Indeed, many medical leaders have expressed indifference or resistance to proposals for reform in the ethics of medical research. They have spoken of "the heavy hand of bureaucracy," of "red tape," even, in some extremes, of "the death of science." New initiatives in this field have come from a few members of the medical establishment,[19] but the chief agent of effective reform has been the government. Perhaps the most powerful agency for reform has been the regulation established by the National Institutes of Health in 1966 that all research it funded thereafter must be approved by a local peer review committee with regard to satisfactory standards of risk-benefits ratios and of informed consent. More recently, in 1975, under the impetus of the Tuskegee scandal, in which black men were being used as subjects in syphilis research without their informed consent, the Congress established the National Commission for the Protection of the Human Subjects of Biomedical and Behavioral Research. Building on the established NIH regulations, as amended since 1966, the commission has the mandate of recommending new protective procedures for human

[16] Jay Katz, ed., *Experimentation With Human Beings* (New York: Russell Sage Foundation, 1972); Bernard Barber et al., *Research on Human Subjects* (New York: Russell Sage Foundation, 1973); B.H. Gray, *Human Subjects in Medical Experimentation* (New York: Wiley-Interscience, 1975); and S.J. Reiser et al., eds., *Ethics in Medicine* (Cambridge, Mass.: MIT Press, 1977).

[17] J.J. Lally, "The Making of the Compassionate Physician-Investigator," *Annals* 437(1978): 86–98. This issue of *Annals* is a special one on *Medical Ethics and Social Change*, Bernard Barber, special ed.

[18] Barber et al., *Research on Human Subjects*.

[19] H.K. Beecher, *Research and the Individual* (Boston: Little, Brown, 1970); S.R. Graubard, ed., *Ethical Aspects of Experimentation with Human Subjects, Daedalus* (Spring 1969).

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subjects not only in general but in such specific problem areas as fetal research, psychosurgery, and the use of prisoners and children as subjects. Although both the NIH regulations and the National Commission have been very much influenced by medical professionals, some physicians and researchers still view them as the intrusions of outsiders, an "encroachment of government on medical practice."[20] Such voices are more concerned for the autonomy of the profession than they are for defects in service to the public welfare. Faced with social change, they sound something like businessmen who were faced with similar social changes and "encroachments" of government in the 1930s.

THE LEGAL PROFESSION

Complaints about defects in public service performance and in effective self-regulation reach a crescendo in the case of the legal profession. Writing a scholarly history of the American legal profession from 1890 to the present, Jerold S. Auerbach entitles his book, *Unequal Justice*.[21] "In the United States," he says, "justice has been distributed according to race, ethnicity, and wealth, rather than need. This is not equal justice."[22] Nor has this injustice been the fault of the less well trained among the lawyers, the "less professional" members of the bar. "The professional elite," continues Auerbach, "bears a special responsibility for this maldistribution. Its members, absorbed with selective client-caretaking for a restricted clientele, have preserved social and economic inequality. Their efforts, in conjunction with the limitations of an adversary process largely dependent upon the ability to pay, have crippled the capacity of the legal profession to provide equal justice under law or to fulfill those paramount public responsibilities that alone can justify professional independence and self-regulating autonomy."[23] Because his evidence shows in detail that the legal profession does not serve the community as a whole but the interest only of one part—the affluent, the powerful, and the ethnically and racially privileged—Auerbach concludes his book with an eloquent call for more public control and regulation. Auerbach's findings and conclusion are supported by a study of "the unseen power of Washington lawyers," a study that calls these lawyers who sit so close to the national seats of power "the other government."[24] "The bar," *Time* magazine quotes Professor Vern Countryman of the Harvard Law School as saying on the eve of the one hundredth annual meeting of the American Bar Association, "is still dominated by shortsightedness and self-interest."[25]

[20] I.H. Page, "What Price Protection?" *Journal of the American Medical Association* 235(1976): 286; I.H. Page, "Ecumenicalism in Medicine and Science—Its Urgent Needs," *New England Journal of Medicine* 297(1977): 215–216.

[21] Jerold S. Auerbach, *Unequal Justice: Lawyers and Social Change in Modern America* (New York: Oxford University Press 1976).

[22] Ibid., p. 12.

[23] Ibid.

[24] M.J. Green, *The Other Government: Unseen Power of Washington Lawyers* (New York: Grossman (Viking), 1975).

[25] *Time*, August 8, 1977.

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As with medicine, public distrust of and hostility to the legal profession has recently expressed itself in a sharply rising rate of malpractice suits.[26] As a result, malpractice insurance rates have risen to such an extent that not only the American Bar Association but "just about every state and local bar association" has set up special committees to consider the "potential crisis in the availability and affordability" of lawyers' malpractice insurance. The rise in the number of legal malpractice suits has occurred not only because of declining public respect for lawyers but also because of changes in the law that make such suits easier and in the willingness of at least a few lawyers to violate the "clubby" collegiality of the law which tends to define such lawyers as traitors to their profession. In a story on "lawyers who sue lawyers," it is reported that no one yet specializes in such legal malpractice suits, as some lawyers specialize in medical malpractice suits, but now at last there are a few lawyers who will take on a few such cases.[27] In smaller communities, where clubbiness among lawyers is easier to enforce, it is very difficult to find a lawyer willing to take on a legal malpractice suit; in large cities, it is a bit easier. Unfortunately, most suits for legal malpractice are settled out of court and with the offending lawyer stipulating, first, that the plaintiff retract his charge of legal malpractice from the record and, second, that the record be sealed to the press and the public. These stipulations prevent effective control of the profession either through public denigration or informal or formal professional sanctions.

Not that such professional sanctions operate very effectively. As with the medical profession, one of the reasons laymen resort to the courts to complain about lawyers is that there is usually nowhere else they can go with their grievances. Although the Lawyer's Code of Professional Responsibility, which is the American Bar Association's official code of ethics, requires lawyers to report cases of professional misconduct that come to their attention, few lawyers do so. And the grievance committees of the local bar associations to which such complaints are supposed to be brought, have not been well staffed, active, or effective in investigating them when they are brought directly by laymen. The offending lawyers have been permitted to use all the forms of legal evasion granted under generous interpretations of due process. In any case, it seems to be only the "small fry," the "less professional" among the lawyers who are ever even brought up before the grievance committees. Quite recently, under the influence of increasing protests from the public and from a small group within the profession itself, especially the younger, "public interest" lawyers, the grievance committees have been making small efforts to reform themselves. The bar associations have granted them more funds to hire more staff and have urged them to do their job more effectively. In some cases, however, the local bar associations have not been willing to go against the particularistic clubbiness that prevails, especially in the establishment bar, and have passed their control responsibilities on to other agencies. In Illinois, for example, the Chicago

[26] See for example, *New York Times*, February 28, 1977; and *Wall Street Journal*, August 17, 1977.
[27] *New York Times*, June 26, 1977.

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Bar Association and the Illinois State Bar Association supported the setting up, in 1973, of the Attorney Registration and Disciplinary Commission by the state's Supreme Court to be responsible for professional self-regulation.[28] The Chicago and state bar associations successfully opposed the membership of laymen on this new commission. Thus, clubbiness is perpetuated, though it is likely that the new commission, with its larger and more professional staff, its visibility and legitimacy, and its removal from the immediate local community, will be more effective than the previous disciplinary agencies.

There remain those who know the legal profession and who doubt that anything less than public disciplinary control with laymen participating will be really effective in maintaining high professional standards. Monroe Freedman, former dean of the law school at Hofstra University and the author of a textbook on legal ethics, says that effective control is not going to come from the bar associations. "The bar," he says, "has never policed itself adequately . . . there's too much self-protection there . . . and too much betraying of a client's interests."[29] A public interest group sponsored by Ralph Nader takes the same position. This group, called Public Citizen, in a 186-page study of the disciplinary procedures of the bar associations in Baltimore, Boston, New York, Philadelphia, and Washington, concludes that only the continuous participation of and pressure from informed citizens will make the legal profession produce better and cheaper service.[30]

THE ACCOUNTING PROFESSION

Accounting is one of the modern professions, not so old or prestigious as medicine or law, but growing in power and consequentiality for the public welfare. Accounting has to do with setting and checking standards of information and accountability in the financial statements of public and private organizations. The 130,000 accountants who are members of the American Institute of Certified Public Accountants and the large firms in which many of them work are responsible, for example, for the reliability of the financial information issued by American corporations, information on which individual investors, pension funds, and various institutions depend to make investment decisions. If this information is unreliable, investors may lose hundreds of millions of dollars, as has happened in some notably scandalous cases detailed by Briloff.[31] Accountants are also responsible for an "independent audit" or certification of the financial information issued by these corporations as the basis on which the government assesses taxes. Unreliable and excessively flexible accounting practices and

[28] M.J. Powell, "Professional Self-Regulation: The Transfer of Control from a Professional Association to an Independent Commission," American Bar Association, mimeo.

[29] *New York Times*, June 26, 1977.

[30] *New York Times*, July 18, 1977.

[31] A.J. Briloff, *More Debits Than Credits: The Burnt Investor's Guide to Financial Statements* (New York: Harper & Row, 1977).

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procedures may cost the government and the public, again, hundreds of millions of dollars. It is obvious that accounting is very much and increasingly affected with the public interest. It is also a profession that is increasingly based on the generalized knowledge created by academic economics and the other social sciences. There is, of course, a range of variation within the profession, from mere "bookkeepers," who perform only the simplest of accounting tasks, to academic professors who are responsible, in some cases, for advancing the theory and standards of accounting.

How well does the accounting profession measure up on the dimensions of public service performance and effective self-regulation? Not very well, according to a recent 1,760-page report of the Committee on Government Operations of the U.S. Senate[32] and to such critics as Professor Briloff.[33] According to the Senate report, the accounting profession is dominated by a few very large firms, the so-called "Big Eight." These firms dominate the profession through their control of the committees, offices, and staff of the key professional organizations, such as the American Institute of Certified Public Accountants, the Financial Accounting Foundation, and the Financial Accounting Standards Board, which set standards for the profession. Though they are supposed to be "independent" professional auditors of the financial statements of America's corporations (the "Big Eight" audit the accounts of 85 percent of the corporations listed on the New York Stock Exchange), these firms often seem to care more about the interests of the corporations than the public welfare. They identify with the corporations rather than the public and join them in lobbying efforts against congressional efforts to reform accounting and corporate reporting standards. Through their domination of the accounting standards boards, the "Big Eight" practice what they call "creative accounting," that is, they adopt standards that have been labelled by outsiders as being so flexible that corporations sometimes seem to be able to call profits losses, and losses profits, depending on which label is most profitable and least taxable. In a considerable number of cases that have later been publicly exposed, accounting firms have aided corporations in deceiving the public and the government.

Among the proposals for reform of this unsatisfactory situation recommended in the Senate report were stronger oversight of accounting practices by Congress, the direct establishment of financial accounting standards by the government itself through a special commission for that purpose, and "participation by all segments of the public" in these tasks. When committee hearings on the report were held, however, the senators finally abstained from recommending any legislation toward these ends at this time. Responding to proposals from the accounting profession's leaders for self-reform, the senators decided to give the profession another chance to make itself more "professional."[34]

[32] *The Accounting Establishment.*

[33] Briloff, *More Debits Than Credits.*

[34] *New York Times*, April 1, 12, 20, June 14, 20, 1977.

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THE ACADEMIC PROFESSION

Though purists among them sometimes resist being so labelled, academic scientists and scholars are "professionals" in high degree by the several criteria specified in our definition. They are the creators and teachers of the most highly generalized knowledge. And they have large effects on the public welfare in a number of ways.[35] They mold the values and ideologies of their undergraduate students; they train the other professionals; their generalized knowledge is essential for maintaining and advancing performance in the practicing professions, thus also indirectly affecting the general public; and they often even have important direct effects on public values and welfare, as when they discover atomic energy, do experiments on human subjects, do recombinant DNA research, or, as writers of school textbooks or as intellectuals, discourse on the nature of man and on man's evolution from animal species. Thus, academic scientists and scholars are a profession by virtue of both their generalized knowledge and their consequentiality for the public welfare. There is, of course, a range of "professionality" among them, extending from routine teachers in marginal colleges to distinguished "stars" in the sciences and the humanities in the great universities.

Even here, however, we find less than perfectly satisfactory service in the public welfare and occasional lapses from effective self-regulation. Means become ends even for academic professionals.[36] For example, in their zealous pursuit of discoveries, academics become heedless of the consequences of those discoveries for the public welfare and of the costs of some of their procedures to the individuals who are the objects and subjects of their research. A great deal of the recent outcry against the abuses of human experimentation by biomedical scientists is against practices located to a considerable extent in premier academic institutions.[37] And when the National Institutes of Health, in its guidelines designed to curb abuses of this kind, required lay participation on the local institutional ethical review boards, this requirement met with hostility in many academic quarters. Academic scientists have done somewhat better about their public responsibility and self-regulation in the case of the possible dangers resulting from recombinant DNA research by taking the initiative, at Asilomar in 1975, of setting up protective standards for all such research. But the same scientists have been at least ambivalent and sometimes hostile when local or national governmental regulations for the dangers of this research have been proposed. Scientists and scholars sometimes assert claims to autonomy that are viewed as unwarranted and "arrogant" by some sections of the public. In the fluoridation controversy, for example, scientists were denounced by political

[35] Rae Goodell, *The Visible Scientists* (Boston: Little, Brown, 1977).

[36] Robert K. Merton, *The Sociology of Science* (Chicago: University of Chicago Press, 1973), p. 262. This essay was originally published in 1938.

[37] Jay Katz, *Experimentation;* Barber et al., *Research on Human Subjects;* and Gray, *Human Subjects.*

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rightists for imposing their views on the public.[38] In the case of the controversy over building nuclear energy plants, the charge is the same, though this time it is made by political leftists. In her study of the recent science textbook controversies, where there was public criticism of the views on evolution stated in biology and social science textbooks, Nelkin points out that for these critics science has become a "symbol of an authoritarian ideology." These critics "express extraordinary resentment of 'scientific dogmatism,' of the 'arrogance' and 'absence of humility' among scientsts." Criticism of "the dominance of scientific values and the role of expertise, as well as demands for increased local participation," she continues, "can be found among people of a wide spectrum of political ideologies. . . . All these groups have expressed disillusion with technology and expertise, and their slogans are 'accountability,' 'lay participation,' and 'demystification of expertise.' "[39]

The public is not radically anti-intellectual or anti-scientist, as some academics fear, but it is ambivalent about the mixed consequences of academic professionalism and it is, increasingly, demanding more lay and democratic control over some of these consequences. Academic professionals who resent or resist an increased measure of public control may well take note of Duncan Macrae's remark: "Democracy, however, requires that the electorate have the ultimate power. Those who value democracy, or fear its erosion, sometimes see scientists as an elite serving special interests, or see science as simply unplanned and uncontrolled."[40]

New Mechanisms for Improved Control and Responsibility in the Powerful Professions

Social change is often hard to come by, and slow. Nonetheless, along with the widespread investigation and criticism of present control and responsibility processes in the powerful professions, there have been many moves toward reform and greater professionalism from both inside and outside the professions, sometimes separately, but also frequently conjointly. Individual professionals, voluntary groups, the foundations, and the state and federal governments have all contributed, often together, in these reform efforts.

Fortunately for professional standards and for the public welfare, there are always a few members of each profession who are dissatisfied with its present performance and who become advocates of reform from within. Among biomedical researchers, Professor Henry Beecher, very much a member of the establishment in that group, became a powerful agent of reform with his exposure of widespread abuses of the rights of human subjects.[41] As with some other

[38] Allan Mazur, "Disputes between Experts," and "Opposition to Technological Innovation."

[39] Nelkin, *Science Textbook Controversies*, pp. 131, 138.

[40] Duncan Macrae, Jr., "Science and the Formation of Policy in a Democracy," *Minerva* XI(1973): 228–242.

[41] Beecher, *Research and the Individual*; Beecher's English counterpart is M.H. Pappworth, *Human Guinea Pigs* (London: Routledge & Kegan Paul, 1967).

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criticisms from within the professions, Beecher's was somewhat weakened by his tendency to see these abuses as the faults of "bad guys" in the profession, not the clear result of defects in the existing system of self-regulation.[42] But internal critics often see these system defects. For example, a group of young lawyers in the Chicago Bar Association, in order to reform the CBA's inadequate system of control over unprofessional lawyers, formed the Chicago Council of Lawyers to bring about needed changes in the system. Powell points out that "the Council was not interested in patching up the existing system; it was clearly beyond repair and was inherently inadequate, controlled as it was by lawyers without reference to members of the public."[43] Eventually the Council was successful in having established a better system, a statewide Attorney Registration and Disciplinary Commission. System defects in the present processes of legal practice and legal education have also been pointed out by Douglas Rosenthal[44] and Jerold Auerbach.[45] Rosenthal's research, done for a Ph.D. at Yale, shows that the typical authoritarian stance that lawyers take toward their clients is no more successful in winning suits than a stance that actively includes the lay client in the prosecution of his case. Rosenthal is a practicing attorney in the U.S. Justice Department. And Auerbach, trained as a lawyer but now a professional historian, indicts the profession for its preference for the powerful in American society and its lack of equal service to the poor and less powerful.

Insider professionals do not limit themselves to criticisms and to proposals for reform. They have been active in setting up agencies for reform of professional obligations. With the financial help of private individuals and the foundations, notably The Ford Foundation,[46] young, liberal members of a number of professions have set up public interest professional organizations to serve the public interest directly and continuously. Public interest law firms have been active in such areas, where there are important public interests that have not been adequately represented before, as the environment, health services, housing, employment, communications, education, and women's and minority rights.[47] What the lawyers started, economists and tax experts and accountants have recently continued. The Ford Foundation has supported the establishment of Tax Analysts and Advocates, the National Association of Accountants in the Public Interest, and the Public Interest Economics Foundation.[48] Finally, with support from their own members and from other sources, academic scientists have established a number of public interest organizations to provide knowl-

[42] John J. Lally and Bernard Barber, "The Compassionate Physician: Frequency and Social Determinants of Physician-Investigator Concern for Human Subjects," *Social Forces* 53(1974): 289–296; Barber, "Compassion in Medicine."

[43] M.J. Powell, "Professional Self-Regulation."

[44] Douglas E. Rosenthal, *Lawyer and Client: Who's in Charge?* (New York: Russell Sage Foundation, 1974).

[45] Auerbach, *Unequal Justice.*

[46] Ford Foundation, *The Public Interest Law Firm: New Voices for New Constituencies.* New York, 1976.

[47] Ford Foundation, *Newsletter,* June 1, 1977.

[48] Ibid.

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edge and guidance on public issues where science matters were important.[49] While most of these are for scientists only, a few include laymen. For example, an organization called Public Responsibility in Medicine and Research has been established in Massachusetts "as a vehicle by which researchers, clinicians, research and health care administrators, attorneys, and members of the lay public can be brought together" in response "to the increasingly complex and sensitive problems currently facing research and related clinical practice."[50]

Nonetheless, all this has not been enough. Establishments, among professionals as among other social groups, are always conservative when what they define as their central values are being subverted, as they see it. The professions have been resistant to both insider and outsider attacks on their absolute autonomy. As a result, outside reforms, especially from a variety of government actions and agencies, have been necessary to supplement and strengthen insider initiatives for reform.

In biomedical research, for example, Beecher's criticisms of abuses of human subjects would not have been effective alone. Medical schools did not set up local peer review committees for the control of such abuses until the National Institutes of Health, on whom they depended vitally for financial support of their research, required them in 1966 to set up such committees on pain, otherwise, of not being funded.[51] Administrative action of this kind by the National Institutes was supplemented in 1973 by legislative action by the Congress in establishing the National Commission for the Protection of the Human Subjects of Biomedical and Behavioral Research. The commission was set up, after the Tuskegee scandal showed that abuses were still occurring, to improve the treatment not only of subjects in general but of certain special classes of them: children, prisoners, and those receiving psychosurgical interventions. In addition to these actions by the government, there have been investigatory commissions that have looked into defects in the medical profession, as in the case of malpractice.[52] Also, as we have seen, various state governments have taken action. The New York State Board of Regents, going against established professional codes, has allowed not only doctors and dentists but also a variety of other professionals whom it regulates to advertise, under specified conditions, to the public.[53] Advertising alone, as Ladinsky has pointed out, will be of limited usefulness in achieving more lay control over the medical profession, but it is a step in that direction.[54]

[49] Nelkin, *Science Textbook Controversies;* Nelkin, "Scientists and Professional Responsibility"; M. Perl, "Public Interest Science," *Physics Today,* June, 1974; J. Primack and F. von Hippel, *Advice and Dissent* (New York: Basic Books, 1974); and J.M. Berry, *Lobbying for the People* (Princeton: Princeton University Press, 1977).

[50] *PRIM&R,* a newsletter, January, 1977.

[51] Barber et al., *Research on Human Subjects.*

[52] U.S. Dept. of Health, Education, and Welfare, *Report . . . On Medical Malpractice.*

[53] *New York Times,* July 29, 1977.

[54] Jack Ladinsky, "The Traffic in Legal Services: Lawyer-Seeking Behavior and the Channeling

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Not only state boards, but the Supreme Court itself has decided, on petition from a couple of lawyers, that professional codes which prohibit advertising by lawyers are illegal.[55] In its 1977 annual meeting, the American Bar Association, with great reluctance, has acceded to this judgment and has formulated rules for advertising by its members.[56] So far as the legal profession is concerned, the federal government has gone beyond mere judgments to definite action to correct defects in public service responsibilities. The Legal Services Corporation of the government's Office of Economic Opportunity is now operating on a budget of $200 million in its provision of legal services to the poor who would not otherwise get them.[57]

Concluding Comment

For the future, we can look to continued efforts for the improvement of professionalism in the powerful professions through a mixture of better self-regulation, continued public concern, and stronger government actions where self-regulation is not sufficient. Professional activities are clearly too important to the public welfare to be left to the control of the specialized professionals themselves. Democratic and egalitarian sentiments in American society will continue to be in tension with the inherent elitism and power of professional knowledge and the associations which build up around it. Those who believe in rational remedies for social problems of this kind will hope that the social sciences will be able, through research and analysis, to make their contribution to constructive accommodations and resolutions of the tensions between egalitarianism and elitism. That tension will not go away. Our best hope is for improving solutions that pay their full respects to both egalitarianism, on the one hand, and specialized and powerful and useful knowledge, on the other.

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of Clients," in L. Brickman and R. Lempert, eds., *The Role of Research in the Delivery of Legal Services* (Washington, D.C.: Resource Center for Consumers of Legal Services, 1976).

[55] *New York Times*, June 28, 1977.

[56] *Wall Street Journal*, August 17, 1977.

[57] *New York Times*, July 31, 1977.

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The Impact of *The American Voter* on Political Science

GERALD M. POMPER

"A teacher affects eternity," according to an old adage with which academics console themselves while facing recalcitrant students. If that is true, a major book affects even more people, for it reaches many teachers, many student bodies, and many populations. Clearly *The American Voter* is such a book. Although this book has never been revised, it is still relevant in describing the behavior of contemporary voters, some of whom were not even born when this research was conducted. It has been seminal in the development of research on electoral behavior particularly and on political science generally. Indeed, it can be viewed as a "paradigmatic" work, setting the boundaries and standards for subsequent research.[1]

Scholars—especially the four authors of this volume—would agree, however, that any book must be subject to continual examination and reappraisal. Now that *The American Voter* has reached voting age, it is an appropriate time to analyze its impact, evaluate its limitations, and suggest new directions in research.

One measure of its impact, if not necessarily its truth, is supplied by the marketplace. The book has sold nearly 100,000 copies in clothbound and paperback editions.[2] It has now been officially certified as a classic by the recent republication of the hardcover edition by the University of Chicago Press.

[1] W. Lance Bennett, "The Growth of Knowledge in Mass Belief Studies: An Epistemological Critique," *American Journal of Political Science* 21 (August 1977): 465–500.

[2] This information has been supplied by Mr. Wayne Anderson, political science editor of the

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Its academic impact can be gauged from textbooks and scholarly journals. Standard textbooks in American government and politics rely heavily on its conclusions. Illustratively, even as late as 1975, Burns and Peltason cite *The American Voter* and its descendants ten times, while the first edition of Sorauf's parties text referred to these works in twenty of fifty footnotes.[3] Even a deliberately critical book such as Dye and Zeigler is dependent on these sources, using them for fifteen of its forty-two citations.[4] Journal articles have shown the same respect to the original work. In a symposium on voting behavior in the *American Political Science Review*, there were sixty-two references to the Michigan research. A later discussion, although centered on other studies, added fifty-nine more references.[5]

Specialists in the field of electoral research have been made into academic catatonics. It is hardly an exaggeration to say that there was but one other original work on American electoral behavior for all of fifteen years after the publication of the work of Campbell, Converse, Miller, and Stokes—the "fearsome foursome," as one of my students described them. Such works as were published were essentially summaries and restatements of the basic findings of the original research.

The one possible exception to the deference paid to this work was V.O. Key's posthumous work, *The Responsible Electorate*.[6] Yet Key sought in that important work to refute what he regarded as distortions of *The American Voter*, not to challenge the original findings. In fact, he praised the book quite fully upon its publication.[7] Moreover, in handwritten notes for the projected final chapter of his own work, he concluded that his research "probably confirms the significance of Angus Campbell's model built on party identification."[8]

Sources of the Impact

To explain the impact is somewhat more difficult than to demonstrate its existence. Obviously some of the influence of the Michigan work is due to its scholarly merits, the care with which the investigations were conducted, the detail and elegance of its presentation, and the revealing truths of its findings. However, good research is not always its own reward; many a village Hamp-

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publisher. The book under consideration is Angus Campbell et al., *The American Voter* (New York: John Wiley, 1960).

[3] James M. Burns et al., *Government by the People*, 9th ed. (Englewood Cliffs, N.J.: Prentice-Hall, 1975); Frank Sorauf, *Party Politics in America* (Boston: Little, Brown, 1968).

[4] Thomas Dye and L. Hermon Zeigler, *The Irony of Democracy*, 2nd ed. (Belmont, Calif.: Wadsworth, 1972).

[5] *American Political Science Review* 66 (June 1972): 415-470; *APSR* 70 (September 1976): 753-849.

[6] V. O. Key, Jr., *The Responsible Electorate* (Cambridge: Harvard University Press, 1966).

[7] V. O. Key, Jr., "The Politically Relevant in Surveys," *Public Opinion Quarterly* 24 (Spring 1960): 54-61.

[8] Key, *Responsible Electorate*, p. 150.

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den rests unnoticed in the country churchyards of scholarship. Aside from quality, there were other reasons for the renown of *The American Voter.* These reasons are to be found in the fit between the book itself and the nature of the times in which it was researched, published, and distributed.

When we speak of the "behavioral revolution" in political science we speak specifically of the decade of the 1950s, and perhaps of the preceding and following five years. The discipline sought to capture the glamour of science, highest after the end of World War II, for itself. It arose in the period of the cold war, when ideological controversy was subordinated to national consensus, encouraging political science to neglect normative questions and to concentrate instead on value-free analyses.[9] The technology of high-speed digital computers had been developed, opening up the possibilities of manipulating large sets of quantitative data. The means to obtain such data had already been demonstrated by the sociologists in two important works, *The American Soldier* and *The People's Choice.*[10] The application of these methods to the study of mass politics was an obvious, indeed inevitable, step.

The American Voter rode the crest of the behavioral revolution, and was in fact its most prominent example. Techniques of sampling were now available to investigate not only the small groups of college students previously favored by psychologists and not only the individual communities probed by Lazarsfeld and Berelson.[11] The entire nation could now be represented by fewer than 2,000 carefully drawn respondents. Findings could not longer be dismissed as representative only of Sandusky, Ohio or Elmira, New York.

This research, moreover, was not only at the right time but also at the right intellectual place. Voting behavior is a subject particularly likely to interest others. As someone has said, everybody is an amateur political scientist, and every political scientist is an amateur voting specialist. Almost all political scientists vote—and think they know something about what happens in the process. Additionally, casting a ballot is the most common political activity of Americans generally, and the fame and fortunes of politicians, advertisers, consultants, interest groups, and others directly depend on accurate knowledge of mass electoral behavior. A large scholarly and national audience therefore was available for an insightful work on voting.

The American Voter was not only insightful, it was insightful in the right way. It appropriately rejected two other modes of interpretation, that is, appropriately for the intellectual climate of its time. It rejected the tradition of armchair speculation on voting which could be traced back at least to Plato's comments about the misguided actions of an inexpert population. Instead of

[9] This point is made by Walter Dean Burnham, "Contributions of the SRC to the Development of Voting Theory" (Paper presented at American Political Science Association Annual Meeting, San Francisco, Calif., September 1975) p. 7.

[10] Samuel Stouffer, *The American Soldier* (Princeton: Princeton University Press, 1949); Paul Lazarsfeld et al., *The People's Choice,* 2nd ed. (New York: Columbia University Press, 1948).

[11] See Bernard Berelson et al., *Voting* (Chicago: University of Chicago Press, 1954).

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speculation, this book presented hypotheses. In place of personal examples, it presented representative data. Rather than moral exhortations, it presented behavioral regularities. Illustratively, armchair speculation had championed the nonpartisan, independent voter, rationally choosing between competing issues and candidates. The virtues of behavioral research were exemplified in the SRC description of the so-called Independent voter. With what joy we empiricists read of the deficiencies of that erstwhile paragon of the civics textbooks![12]

Secondly, the book rejected the sociological interpretation of voting, and thus saved the self-respect of political scientists. Empirical research on voting to this date had been conducted by sociologists invading our turf. With the appearance of *The American Voter*, electoral analysis became again the province of political scientists, even if, like the Michigan authors, they had to be recruited from social psychology or mathematics. No longer would politics be seen as only a reflection of social position, nor need we any longer believe, with Lazarsfeld, that "a person thinks, politically, as he is socially."[13] Instead, we would now examine "the politically relevant in surveys," in Key's phrase. The satisfaction, even relief, felt in the discipline is exemplified in the work of Daudt, who had completed a severe criticism of voting studies in 1960. Upon publication of *The American Voter*, he wrote an epilogue, fulsomely praising the new book as the answer to his previous criticisms of sociological determinism.[14]

Material and organizational factors also help to explain the impact. The significance of the first work of the Survey Research Center led to grants for further research, which in turn led to more findings, more impact, more grants, etc. By making their data readily available, the Michigan group certainly advanced the cause of scholarship. Their generosity was a signal service to all social scientists, perhaps most of all those young persons needing to publish quickly to gain promotion. The latent function of the availability of grant funding and the data, however, was to create a national corps of SRC disciples, who would have little intellectual choice but to accept the basic premises and methods of SRC.

As Michigan drew, trained, and placed the most outstanding graduate students in this subfield, these premises and methods would become the standard explanation of voting throughout the classrooms of America's colleges and universities. This development was furthered by the creation of the Inter-University Consortium for Political Research, which began its valuable work as a distribution center for the results of the national voter surveys. Data were now available to all, for a once-modest fee, and would be imbibed directly by impressionable undergraduates. The summer training programs then brought faculty and graduate students to Ann Arbor for personal contact with the "fearsome foursome" and for socialization into their axioms and techniques.

[12] Campbell et al., *American Voter*, p. 143.
[13] Lazarsfeld et al., *People's Choice*, p. 27.
[14] H. Daudt, *Floating Voters and the Floating Vote* (Leiden: Stenfert Kroese, 1961).

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In no way do I mean to suggest that these efforts were ill-conceived, deleterious, or conspiratorial. The important work of conducting national surveys necessarily had to be centralized, and Michigan was the appropriate place to locate the enterprise. It has been done openly, seriously, and conscientiously. The entire discipline has benefited enormously. My point is that the inevitable effect was to create an intellectual quasi-monopoly in thinking about voting behavior. This effect is similar to that of the Bell system in telephone service. Here too a quasi-monopoly is technologically determined. It results in the best telephone service in the world, but it is also a constraint on trade—appropriate, useful, and not conspiratorial, but still a constraint.

We may also look to the overall tone of the volume to explain its impact. There is a distinct elitist appeal to this work. It uses modern statistical methods, comprehensible to the trained specialist but somewhat mysterious to the average voter. In so doing, it reinforces the claims of social scientists to be expert professionals, a guild with secrets unknown to the uninitiated. Moreover, the findings are not basically complimentary to the democratic elector. He, and apparently more often she,[15] is not well informed, has poorly developed attitudes toward politics, and is heavily dependent on traditional loyalties in making voting decisions. Exceptions to these general descriptions are found principally among the well-educated, that is, among political scientists. To be sure, the book was not written by snobs or as an effort to promote snobbism, but its appeal may partially lie in its elitist implications.[16]

Finally, in one effort to be fair, one must acknowledge that the impact of *The American Voter* is also due simply to its scholarly merits. Its authors accumulated their data carefully. They allowed, even encouraged, criticism by making that data available to persons of different outlooks. They provided the basis for extended comparisons across time by accumulating a unique time-series archive, and across space by pursuing similar studies in other nations. In contrast to some later interpreters, their conclusions were stated precisely and with many necessary qualifications. Building on their sound empirical work, they went on, at least in their concluding chapters, to attempt the development of empirical theories. By providing these model elements of good scholarship, their impact has been not only great, but good.

The Present Challenge

The influence of a work of scholarship can also be gauged by the controversy it creates and the responses it stimulates. By this test, *The American Voter* is currently being born again, which is perhaps appropriate in the present con-

[15] Sexism might be suspected in the choice of interviews illustrating voter levels of conceptualization: For the "higher" levels A and B, six men and six women are selected. For the "lower" levels C and D, only two men but nine women are presented. See Campbell et al., *American Voter*, pp. 228–249.

[16] See Jack Walter, "A Critique of the Elitist Theory of Democracy," *American Political Science Review* 60 (June 1966): 285–295.

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text of American politics. At least five major works have been published in the last two years which are direct challenges to the original volume.[17] The most prominent of these new books takes its title, *The Changing American Voter*, directly from the Michigan study and is actually dedicated to those four authors.

These books, and a multitude of journal articles, directly challenge or refute many of the specific findings of *The American Voter*. A brief canvass of this literature provides the following list of twenty-five disputed propositions drawn from the original work, grouped under four headings.[18]

Partisanship

1. Since citizens are largely supportive of the political parties, party identification is the principal determinant of the vote.[19]
2. Independents are less knowledgeable and politically active than partisans.[20]
3. Most voters consistently identify with the same party across time.[21]
4. Party images are consistently distorted by the respondent's party identification.[22]
5. The likelihood of defection in the vote is inversely related to the strength of partisanship.[23]
6. In any given election, the bulk of the vote is from standpatters who consistently choose the same party, with change coming predominantly from less interested voters.[24]
7. Party identification is largely an affective, psychological, and traditional attachment rather than a choice based on policy agreement.[25]
8. Partisan loyalty increases steadily with age.[26]

[17] Herbert Asher, *Presidential Elections and American Politics* (Homewood, Ill.: Dorsey Press, 1976); David Knoke, *Change and Continuity in American Politics* (Baltimore, Md.: Johns Hopkins University Press, 1976); Warren Miller and Teresa Levitan, *Leadership and Change* (Cambridge, Mass.: Winthrop Publishers, 1976); Norman Nie et al., *The Changing American Voter* (Cambridge: Harvard University Press, 1976); Gerald Pomper, *Voters' Choice* (New York: Harper & Row, 1975).

[18] In footnotes 19-43 the first reference is to the original statement of the proposition in *The American Voter* (hereafter cited *AV*). The next references are to recent research disputing that propostion.

[19] *AV*, chap. 6; Miller and Levitan, *Leadership and Change*, chap. 5; Arthur Miller et al., "A Majority Party in Disarray," *American Political Science Review* 70 (September 1976): 753-778; Nie et al., *Changing American Voter*, chap. 4.

[20] *AV*, pp. 134-135; Walter Dean Burnham, *Critical Elections and the Mainsprings of American Politics* (New York: Norton, 1970), pp. 123-131; Pomper, *Voters' Choice*, pp. 31-35; William Flanigan and Nancy Zingale, *Political Behavior of the American Electorate*, 3rd ed. (Boston: Little, Brown, 1975), chap. 3.

[21] *AV*, p. 148; Edward C. Dreyer, "Change and Stability in Party Identifications," *Journal of Politics* 35 (August 1973): 712-722.

[22] *AV*, pp. 128-136; Pomper, *Voters' Choice*, chap. 7; Richard Trilling, *Party Image and Electoral Behavior* (New York: John Wiley, 1976), chap. 8.

[23] *AV*, pp. 136-142; Theodore Macaluso, "Parameters of 'Rational' Voting: Vote Switching in the 1968 Election," *Journal of Politics* 37 (February 1975): 202-234.

[24] *AV*, chap. 4 and p. 148; Key, *Responsible Electorate*, chap. 2.

[25] *AV*, pp. 146-150; Samuel Popkin et al., "Comment: What Have You Done For Me Lately? Toward an Investment Theory of Voting," *American Political Science Review* 70 (September 1976): 790-792.

[26] *AV*, pp. 161-167; Paul Abramson, "Generational Change in American Electoral Behavior," *American Political Science Review* 68 (March 1974): 93-105.

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Issues

9. The parties present no clear ideologies or set of policy stances to the electorate.[27]
10. Issue orientations are of relative insignificance in the voting decision.[28]
11. No more than a tenth of the voters, even using generous criteria, can describe the parties in ideological terms.[29]
12. The policy bases of the parties and the vote consist primarily of economic issues originating in the Depression of the 1930s.[30]
13. Ideologies, or coherent patterns of belief across issue areas, are largely absent in the mass electorate.[31]

Social Factors

14. Social class has steadily declined as an influence on the vote.[32]
15. Farmers are the least politically involved of all occupational groups.[33]
16. Social mobility is not associated with changes in party identification.[34]
17. Class-based voting is most evident among persons with strong party identification.[35]
18. Religion has little independent effect on party vote.[36]
19. Level of education is the basic explanation of coherent political attitudes.[37]
20. Low psychological motivation among Negroes (not blacks) is a partial explanation for low levels of political activity.[38]

[27] *AV*, pp. 179–187; Gerald Pomper, "From Confusion to Clarity," *American Political Science Review* 66 (June 1972): 415–428.

[28] *AV*, pp. 256–265; Nie et al., *Changing American Voter*, chap. 10; David Repass, "Issue, Salience and Party Choice," *American Political Science Review* 65 (June 1971): 389–400.

[29] *AV*, chap. 10; John Pierce, "Party Identification and the Changing Role of Ideology in American Politics," *Midwest Journal of Political Science* 14 (February 1970): 25–42; J. O. Field and R. E. Anderson, "Ideology in the Public's Conceptualization of the 1964 Election," *Public Opinion Quarterly* 33 (Fall 1969): 380–398.

[30] *AV*, chap. 10; Miller and Levitan, *Leadership and Change*, chap. 4; Alden Raine, *Change in the Political Agenda* (Sage Professional Papers in American Politics, 1977); Richard Boyd, "Popular Control of Public Policy," *American Political Science Review* 66 (June 1972): 429–449.

[31] *AV*, chap. 9; Nie et al., *Changing American Voter*, chap. 8; James Stimson, "Belief Systems: Constraint, Complexity and the 1972 Elections," *American Journal of Political Science* 19 (August 1975): 393–418.

[32] *AV*, pp. 345–361; Robert Alford, *Party and Society* (Chicago: Rand McNally, 1965), chap. 8; Richard Hamilton, *Class and Politics in the United States* (New York: John Wiley, 1972), chap. 12.

[33] *AV*, pp. 404–408; Michael S. Lewis-Beck, "Agrarian Political Behavior in the United States," *American Journal of Political Science* 21 (August 1977): 543–565.

[34] *AV*, p. 45; James A. Barber, Jr., *Social Mobility and Voting Behavior* (Chicago: Rand McNally, 1970), chap. 6.

[35] *AV*, pp. 365–368; Pomper, *Voters' Choice*, chap. 3.

[36] *AV*, pp. 301–307; Knoke, *Change and Continuity in American Politics*, chap. 2.

[37] *AV*, pp. 250–256; Norman Nie and Kristi Andersen, "Mass Belief Systems Revisited," *Journal of Politics* 36 (August 1974): 566–571; Walter Dean Burnham, "Theory and Voting Research," *American Political Science Review* 68 (September 1974): 1002–1023.

[38] *AV*, p. 279; Donald Matthews and James Prothro, *Negroes and the New Southern Politics* (New York: Harcourt, Brace and World, 1966), chap. 10; Sidney Verba and Norman Nie, *Participation in America* (New York: Harper & Row, 1972), chap. 10; Paul Beck, "Partisan Realignment in the Postwar South," *American Political Science Review* 71 (June 1977): 477–496.

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Electoral Trends

21. Party realignments occur infrequently and as the result of deep, catastrophic, social crises.[39]
22. Elections can be explained by six relatively independent components of the voting decision.[40]
23. Aside from voting, Americans barely participate in political activities.[41]
24. Elections carry no policy mandates.[42]
25. There was a sharp break in the direction of party loyalties occasioned by the Great Depression.[43]

Admittedly, all of these propositions are not equally important and all of them have not been falsified by more recent research. In their entirety, however, they represent a significant part of the findings of *The American Voter*, while the contradictory research cited is of sufficient scope and depth to make the original work largely irrelevant except as a historical monument. We would therefore expect it to be of diminishing impact. New works are likely to take over the intellectual leadership of the field. The republication of the original hardcover edition may therefore be honorific, but in a way analogous to the erection of a gravestone.

However, reports of the death of *The American Voter* are probably exaggerated. Although I have cited twenty-five propositions of doubtful validity drawn from its pages, many would argue that these propositions are still true, and certainly many of them continue to be repeated in textbooks[44] and ingested by students. This book provided us with our basic socialization into the study of voting behavior and it is difficult to change one's early beliefs. Inertia alone will insure a continued hold on our minds of these ideas. Moreover, the quality of the work and the skill of its authors will further strengthen that hold.

Yet, we cannot overlook the weaknesses of *The American Voter* if we are to carry forward the scholarly inquiry these authors initiated. Beyond the truth or falsity of any specific propositions, we should be aware of the general points of attack.

[39] *AV*, pp. 531–538; Everett C. Ladd, Jr. and Charles Hadley, *Transformation of the American Party System* (New York: Norton, 1975), pp. 332–344; James Sundquist, *Dynamics of the American Party System* (Washington, D. C.: Brookings Institution, 1973), chaps. 1, 13, 14.

[40] *AV*, pp. 128–136; William Shaffer, "Partisan Loyalty and the Perceptions of Party, Candidates and Issues," *Western Political Quarterly* 25 (September 1972): 424–433.

[41] *AV*, pp. 91–93; Verba and Nie, *Participation in America*, chap. 2.

[42] *AV*, pp. 541–548; Gerald Pomper, *Elections in America* (New York: Dodd, Mead, 1968), chaps. 7, 8; Boyd, "Popular Control of Public Policy"; John Sullivan and Robert O'Conner, "Electoral Choice and Popular Control of Public Policy," *American Political Science Review* 66 (December 1972): 1256–1268.

[43] *AV*, pp. 150–160; Kristi Andersen, "Generation, Partisan Shift, and Realignment: A Glance Back to the New Deal," in Nie et al., *Changing American Voter*, chap. 5; Philip Converse, *The Dynamics of Party Support* (Beverly Hills: Sage, 1976), 132–135.

[44] E.g., Judson James, *American Political Parties in Transition* (New York: Harper & Row, 1974), pp. 153–154 on ideology and voting.

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First, the research was weak in concept definition. The most basic variable, party identification, is illustrative. There is no clear basis in *The American Voter*, or later research, for asserting that the responses to the standard questions on party loyalty actually reach a basic psychological attachment to the Republicans or Democrats. The only test of this concept has been behavioral regularity in voting, but the concept is claimed to be distinct from that behavior. Furthermore, partisanship is claimed to be an ordinal variable, ranging from strong Democrat through a variety of Independents to strong Republicans. In fact, there are few clear indicators of ordinality in this measure. Indeed, the concept is so much in need of redefinition that a national conference has been convened for precisely this purpose.

Lack of clarity in concepts has led many researchers into difficulty when they attempt to replicate the findings of *The American Voter* or to extend their methods to the analysis of later elections. Thus, the original partitioning of the electorate into four levels of conceptualization was a brilliant individual effort by Philip Converse.[45] But his lesser successors have found it difficult to retrace his steps, or to verify his results.[46]

A second defect of this work was its time period. As the authors have since acknowledged, the 1950s were a time of unusual political quiescence. The passive, compliant, nonideological voter they portrayed was probably at one extreme of a possible scale of political activity.[47] Of course, we cannot fault them for living and doing their research in that particular decade. But there was more to the problem than simply the accident of time. The findings, and particularly the overall conclusions, of *The American Voter* were not presented as the results of a given era, but as relatively long-term truths about the characteristic quality and behavior of the United States electorate. The possibilities of change were hardly discussed, and the brief consideration of such possibilities was only in the context of unexpected catastrophies such as civil war or a major depression.

The book was static in its very outlook, providing no basis even for speculation about change. This work would have led one to predict not black political resistance in the South—but its opposite. It would have led one to predict not voting shifts on the basis of a foreign war—but its opposite. It would have led one to predict not voter responsiveness to the initiatives of political leaders—but its opposite. Given their static stance, these authors could not predict, or expect, or explain "changes in the quality of political life, changes in political

[45] Campbell et al., *American Voter*, chap. 10.

[46] See David Repass's paper, "Levels of Rationality Among the American Electorate" (Paper presented at the American Political Science Association Annual Meeting, Chicago, Ill., September 1974).

[47] Donald Stokes, "Some Dynamic Elements of Contests for the Presidency," *American Political Science Review* 60 (March 1966): 19; Philip Converse, "Public Opinion and Voting Behavior," in Fred Greenstein and Nelson W. Polsby, *Handbook of Political Science*, vol. 4 (Reading, Mass.: Addison-Wesley, 1975), pp. 89–111.

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leadership, and changes in patterns of voting" until long after these changes were startlingly evident.[48]

This problem is related to a more general one of behavioral research, of which *The American Voter* is a leading example. The emphasis of this research is necessarily and properly on observable and preferably measurable phenomena. The movement has done much to make the study of politics more scientific, to make theories explicit and testable, and to remove many false notions and "ghosts" from the field. The discipline is certainly better for its influence.

But the concentration of research on the observable also means that many important questions may be slighted. Nonobservable behaviors are not only not observed; they are also not considered or drawn into speculation. The Civil Rights movement of the 1960s could not be observed in the 1950s, but a speculative mind might have dwelt on the possible significance of the Montgomery bus boycott of 1956. The Vietnam protests could not have been observed when no American soldier had touched foot on that land, but one might have considered more fully the implications of the vote for Eisenhower in the midst of the Korean War.[49] Certainly a behavioral work need not be unimaginative, but the nature of the enterprise does not encourage a futuristic outlook. Prediction will tend to be based on the continuity of the observable present. Yet, when change is the most certain factor, such predictions are likely to be as fallible as those of *The American Voter*.

A final problem with this work is that basically it has no theoretical foundation. Its famous "funnel of causality"[50]—significantly omitted from the paperback edition—is essentially a classification of variables, rather than a set of causal propositions. To quote a critic, "The SRC substituted a *strategy* for a *theory*. . . . Researchers have operated with a few empirical regularities and proceeded to report a large number of high correlations among them. They failed to consider either alternative or comprehensive theories."[51]

The absence of theory from the Michigan work has restricted its impact. While studies of American voting behavior have been embarrassingly dependent on this source, it has had very little influence beyond this one field. One might have thought, for example, that this major work on voting would have some spillover to the study of political parties. Yet it is hardly cited in the textbook chapters on parties, even in the same books that rely heavily on its discussions of voting. Strikingly, the best textbook in the field cited the SRC more frequently than any other source in regard to voting, but provides not a single reference to the Center's work when it deals with parties.[52]

[48] Miller and Levitan, *Leadership and Change*, p. 60.

[49] Data for this examination could be found in the earlier SRC work by Angus Campbell et al., *The Voter Decides* (New York: Row, Peterson, 1954).

[50] Campbell et al., *American Voter*, chap. 2.

[51] Peter B. Natchez, "Images of Voting: The Social Psychologists," *Public Policy* 18 (Summer 1970): 586–587.

[52] Sorauf, *Party Politics in America*. Burns et al., *Government by the People* (note 3) make but three such references in their chapter on parties; Dye and Ziegler, *Irony of Democracy* (note 4) make none.

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Similarly, one would have thought that a major work on American politics would be of interest in other countries and in the development of theories of comparative politics. It seems significant, however, that *The American Voter* has never been translated into a foreign language and that its foreign sales are less than 5,000 over its publishing life. Rarely, if ever, will one find a reference to the work in journal articles on other political systems or in works of empirical theory.

To be sure, *The American Voter* has had its influence in studies of the politics of other nations. The Michigan authors themselves have been prominent in the extension of survey research around the world, and we have increased our knowledge of comparative politics through their research in Great Britain, France, Norway, and elsewhere.[53] Yet, even while the methods of the original work are imitated around the globe, its basic premise of the importance of party identification is disputed. Even as we accumulate more data on voting behavior in a variety of nations, we do not approach a general theory of politics.

The result of these accumulated deficiencies has been a theoretical attack on *The American Voter,* based on both its normative and empirical theories. The authors have been criticized as antidemocrats.[54] The attack is unfair, I believe, but they presented others with the opportunity because they seemed uninterested in the ultimate normative import of their findings. More convincing has been the attack on the empirical theory, or lack of theory, in the work of the SRC.[55] Since these authors did not deal with theory sufficiently, others have now attempted to fill the gap. This is probably the most important next step in the development of voting research.

Future Directions

It is now time to move beyond *The American Voter.* It has had an immense impact, but it may now be retarding development of the field. If we continue to look backward to this monument of the past, we may stumble over future paths. Those paths are reasonably clear, and the way in fact is being trod by some of the original four authors.

Methodologically, we need to develop more complex measures and to ground these measures on clearer definitions of concepts. A new index of party loyalty should be more fully tied to behavior, and should be multidimensional. The concept of political efficacy needs to be subdivided into separate concepts of subjective competence and systemic responsiveness, as suggested by Con-

[53] Among the leading examples are Angus Campbell et al., *Elections and the Political Order* (New York: John Wiley, 1966); David Butler and Donald Stokes, *Political Change in Britain* (New York: Macmillan, 1969). For an extended analysis, see I. Budge, I. Crewe, and D. Farlie, eds., *Political Identification and Beyond* (New York: John Wiley, 1976).

[54] Walker, "A Critique of the Elitist Theory of Democracy"; Peter Bachrach, *The Theory of Democratic Elitism* (Boston: Little, Brown, 1967).

[55] Popkin, "Comment"; Burnham, "Contributions of the SRC to the Development of Voting Theory."

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verse.[56] Issue voting can be better analyzed through proximity measures than by the simple cross-classifications originally used.[57]

These changes need to be made part of a recognition of the dynamic character of elections, and of the importance of events and leadership on mass response.[58] Part of this new recognition will be acknowledgement of the importance of issues to voting, a concession that voting decisions are, like other political decisions, about policies and their effects on groups and individuals. An acceptance of the importance of issues to the voting, if not grudging or churlish, will also do much to revive the legitimacy of democratic participation, at least in the minds of academicians.

Most generally, we need to see voting again as part of a political system, rather than as only an individual-level behavior. We must be more cognizant of its effect on the total system. We also need to view it more within a general system. Methodologically, this may mean more awareness of the impact of areal variables on individual behavior, in the tradition of Durkheim. It means as well more consciousness about large social trends, such as the movements of blacks and women, or the cultural effects of a postindustrial society. Ultimately, it means a consistent concern for theory.

Whatever the new trends in voting research, we can be sure that *The American Voter* has had and will have an impact. It has been indeed a seminal work; and if there have been failures, we should remember that only one sperm in ten million achieves fertility. I suspect that future voting analysts visiting Ann Arbor will come with the reverential and nostalgic attitude that once was true of behaviorists visiting Charles Merriam's haunts at the University of Chicago. They may even offer a poetic remembrance to the pioneering days in which *The American Voter* was written:*

> Joy was it in that day to be alive —
> But to be young was very heaven.

[56] Philip Converse, "Change in the American Electorate," in Angus Campbell and Philip Converse, *The Human Meaning of Social Change* (Beverly Hills, Calif.: Sage, 1972), pp. 302–337.

[57] David Kovenock et al., *Explaining the Vote*, Institute for Research in Social Sciences, University of North Carolina (Chapel Hill, N.C., 1973), Part 1, pp. 1–30.

[58] The importance of these factors had indeed been recognized by the Michigan authors in recent works, such as Converse, *The Dynamics of Party Support;* Miller and Levitan, *Leadership and Change;* and Stokes, "Some Dynamic Elements of Contests for the Presidency."

* This article is a revision of a paper presented at the 1977 meetings of the Social Science History Association in Ann Arbor. I am grateful to William Flanigan for the invitation to prepare the paper.

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Spain Tries Democracy Again

MICHAEL ROSKIN

In 1977 the Spanish electorate was out of practice, but it limbered up quickly. Only Spaniards over sixty-four had previously cast a democratic ballot; it had been forty-one years since Spain's last free elections, and in 1936 the minimum voting age was twenty-three. Yet despite lack of voting experience, turnout in 1977 was 79 percent. Faced with a confusing number of parties whose programs sounded almost alike, the Spanish electorate on June 15 produced a thoroughly West European result. A *sopa de letras* of 161 parties shook down to two large parties and three small ones.

Such a "shakedown" or "first-time" election was bound to evince a certain groping quality. Some parties had trouble locating their natural constituencies; many were mistaken in even presuming they had constituencies. A "disjunction" between electorate and parties occurred in some cases. One such party caught off its electoral base, the Christian Democrats, simply committed suicide.

There was initial confusion among voters. Six weeks before the election, one-third who said they would vote couldn't say for which party. But a week before elections this was down to one-ninth. By the time they got into the voting booth, the undecideds appear to have largely divided between the two big parties.

The elections were a shakedown in other ways, too. The campaign was characterized by an extraordinary amount of personal jealousy among leaders, the jealousy of too many political figures struggling to establish themselves in a political spectrum that could hold only a few. The returns left some odd men out.

Extremist parties found little support. The openly fascist *Alianza Nacional* won not a single seat. Likewise Maoist, Trotskyist, and Anarchist *grupusculos*

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won only 2.4 percent and two seats, representing the far Left in the Basque country and Catalonia. The enemy twins—the *Alianza Popular* on the Right and Communists on the Left, who won nearly the same small percentages—showed the limits of both neofrancoism and Eurocommunism.

Background to the Elections

American images of Spain are often overly romantic and dated. Spain is the world's ninth industrial power and with a 1976 per capita GDP of $2,900, one of the world's richer countries. Spain is no longer largely agrarian. The proportion of Spaniards working on the land plummeted from one-half the total work force in 1950 to one-fifth in 1975.[1] The gap between rich and poor has been bridged; one study found that Spanish society in 1970, based on professions, was 53.9 percent middle class.[2]

By the time Franco died in late 1975, Spain had become a modern country in every respect but the political. In nineteen months Spain was turned into a democracy, *franquismo* overthrown from the top. King Juan Carlos, trained under Franco's eye in Spain's military academies to continue the system, set in motion and backstopped a process to phase it out.

In July 1976, the king fired Prime Minister Arias, a francoist holdover, and picked for the post a young unknown, Adolfo Suárez, whose only cabinet experience had been after Franco's death as nominal head of the anachronistic "National Movement" which by that time was unimportant. Charges that Suárez is a francoist make little sense, for in a few months as prime minister he dismantled much of the francoist structure: the Movement, the phoney "vertical syndicates," and even the old unicameral Cortes (parliament) which had been largely chosen by Franco.

Why didn't the Cortes stop him? They couldn't. One of the ironies of Franco's legacy was the absence of cabinet responsibility to parliament; Suárez was responsible only to the king. Many deputies also held government jobs and were thus vulnerable to government arm-twisting. Further, many Cortes members, who grouped themselves into the right-wing Alianza Popular, thought they could set up an electoral system to favor a continuation of francoism with a democratic gloss. They cooperated in their own political demise. In this whole process Suárez showed himself to be a breathtakingly clever manipulator, outfoxing opponents of both the Right and the Left.

The king's role in Spain's democratization was critical, especially at the time the Communists were legalized. Carefully maintaining his ties with officers—Juan Carlos notes that they used to shower together—the king retained the loyalty of the military. At least one conservative older general was sacked and

[1] Amando de Miguel, *Recursos Humanos, Clases y Regiones en España* (Madrid: Cuadernos para el Diálogo, 1977), pp. 42–43.

[2] See José Félix Tezanos, *Estructura de Clases en la España Actual* (Madrid: Cuadernos para el Diálogo, 1975), p. 44.

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many senior officers were bitter over the legalization of the Communists, but the army stayed calm. Another irony of Franco's legacy was that the king, who had been trained by the military, used his army connections to keep the armed forces *out* of Spanish politics.

The New Cortes

The Suárez government set up a referendum in December 1976 that approved elections for a new bicameral Cortes—a Congress of Deputies of 350 members and a Senate of 207 elected members plus up to one-fifth of this number (that is, up to forty-one additional senators) named by the king. Each of Spain's fifty provinces got four senators. A voter checked *three* individual names (with party identifications) for senator; the top *four* in each province won. The system penalized smaller parties. Of the 207 elected senators, 105 were Centro, forty-seven Socialist, three Communist, two Alianza Popular, the rest scattered among small parties and independent candidates. The forty-one senators named by the king (on election night) were mainly lawyers and professors, generally centrist but with many mavericks among them.

Seats for the Congress were nearly proportional to population except that every province got a minimum of three deputies. The two most populous provinces, Madrid and Barcelona, were assigned thirty-two and thirty-three seats, respectively. The three-seat minimum, as with the Senate's four seats per province, overrepresented rural provinces. For the Congress, each voter selected one party list; seats were then apportioned by the d'Hondt method. A threshold rule at the province level disqualified any party that failed to reach 3 percent. In provinces with five deputies or fewer—and there are twenty-one of these—the system functioned almost as a majority rather than PR system: all but the two big parties were squeezed out. Accordingly, as we see in Chart 1, the larger parties were overrepresented in the Congress at the expense of the smaller ones.

The postelectoral subsidy also worked to the advantage of larger parties. Each seat won—Congress or Senate—would bring the winner 1 million pesetas (about $15,000 at that time) plus 45 pesetas (about 67¢) for each vote for a congressional party list that won at least one seat and 15 pesetas (about 22¢) for each vote for a winning senator. What this meant in practice was that parties that could show banks certainty of repayment could get loans based on expected subsidies.

The Left had several points to complain about: The approximately one million Spaniards working north of the Pyrenees could vote only with great difficulty by absentee ballot. Unquestionably their votes would have gone overwhelmingly to the Left, especially to the Communists who are active among Spaniards working abroad. By holding the voting age at twenty-one, instead of eighteen, which is now nearly universal in West Europe, the Suárez regime kept out more than 1.5 million younger Spaniards whose vote would have

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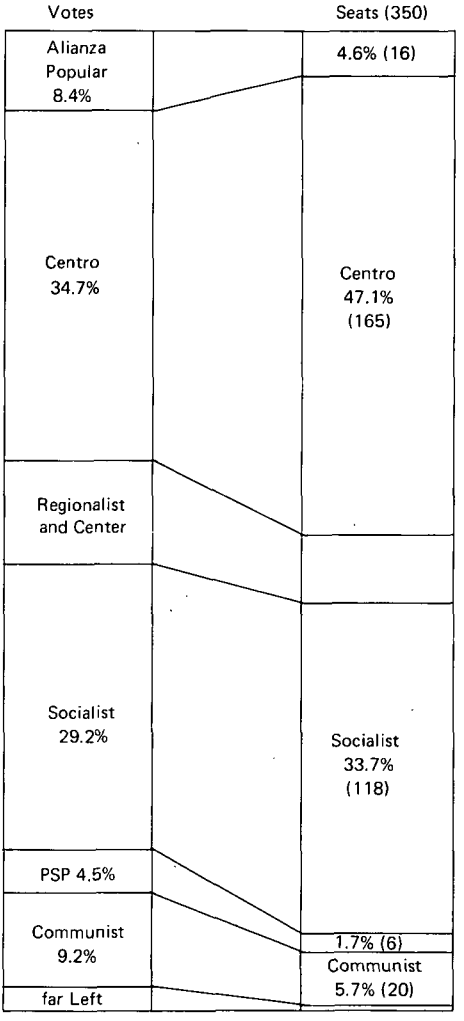
CHART 1

Congress of Deputies, 1977



favored the Left. The very brevity of the campaign, officially three weeks but in practice extending several weeks before the May 24 legal commencement, also favored incumbent Suárez at the expense of newly legalized leftist parties.

Was the electoral system biased? Critics stressed it would underrepresent the Left parties by dint of overrepresenting rural provinces. These provinces did tend to go conservative, to the benefit of Suárez's Democratic Center. Newspapers reported that provincial power structures—themselves government

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TABLE 1

Province Size and Centro Advantage

	pop.	*deputies*	*UCD vote*	*UCD seats*	*% seats*
5 smallest provinces					
Avila	186,183	3	67.5%	3	100%
Guadalajara	130,950	3	47.8	2	67
Segovia	146,703	3	57.7	2	67
Soria	100,421	3	57.7	3	100
Teruel	149,666	3	49.4	2	67
5 largest provinces					
Barcelona	4,536,057	33	15.1%	5	15%
Madrid	4,520,189	32	32.2	11	34
Valencia	1,941,412	15	31.1	5	33
Seville	1,359,664	12	32.2	5	42
Vizcaya	1,212,244	10	15.9	2	20

appointed—were encouraging a Centro vote, sometimes with promises of jobs.[3] Some called this *caciquismo* after the old *caciques*, the rural political bosses who used to deliver the required votes. But to say that a Spanish province is rural does not say very much. The rural provinces in the northern half of Spain, for example, are characterized by smaller, owner-operated farms, and people there have long been deeply and naturally conservative. The provinces in the southern half are characterized by large estates worked by tenants or hired labor. Here the owners may be reactionary and the farm workers radical. Under these circumstances, if a rural province goes conservative, is it because its voters *are* conservative or because they have been pressured by *caciques*? It's hard to tell.

A comparison of the five most- and five least-populous provinces is instructive (Table 1). In the latter, Suárez's Centro got either all three or two of the three seats. In the former, the Centro won proportionally far fewer, from less than one-half (Seville) down to one-seventh (Barcelona). Rural overrepresentation gave Suárez a big lead when Congress seats were apportioned. With 34.7 percent of the vote, the Centro took 47.1 percent of the seats (Chart 1). Still, the Socialists could not complain too bitterly; with 29.2 percent of the national vote they got 33.7 percent of the seats. The d'Hondt system favored both. The d'Hondt system *plus* the three deputies per province minimum favored the Democratic Center. The losers were the smaller parties that won less than 10 percent of the national vote.

[3] "Caciquismo e influencias ante el proceso electoral," *El País*, 12 June 1977, p. 24, and 14 June 1977, p. 24.

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THE CENTRO

The Democratic Center (*Unión de Centro Democrático*) is a cross between a Giscardian UDF (of the 1978 French elections) and an Italian *Democrazia Cristiana*. A catchall grouping of twelve miniparties, it resembled the former in that it was not yet a party but an electoral alignment of Center and Right factions rallying around an incumbent and the latter in that after it formally became a party—in August, after the elections—it lacked programmatic coherence.

In early 1977 several small centrist parties—*Democracia Cristiana, Liberales, Partido Popular, Partido Social-Democrata* and others—coalesced into a Centro Democrático to compete on better terms in the upcoming elections. Soon this alignment took on the coloration of a government grouping and then, just five days before closing date, Prime Minister Suárez, as had been expected, declared he would run as head of the Centro. Suárez's people, *suaristas*, were inserted into electoral lists all over the country. The UCD took on new life; it suddenly had leading it the bright young (forty-four) man of Spanish politics who dismantled francoism and gave Spain free elections. Suárez was genuinely popular, and his Centro became the leading party in all surveys.

But the Suárez takeover also brought problems, provoking cries of opportunism and giving the Left a natural target. Several important Christian Democrat and Liberal figures campaigned against the Centro. Indeed, one of Suárez's most damaging adversaries had been one of the early Centro leaders: José María de Areilza, dapper former ambassador to Washington, self-styled "civilized conservative," cofounder of the Popular Party, and a peppery orator. Before the king's selection of Suárez in July 1976, Areilza had had a fair chance of becoming Spain's prime minister. The extreme bitterness with which he attacked Suárez suggested personal jealousy, perhaps justified. Running for no office himself, Areilza lent his sharp tongue to the anti-Suárez cause. Calling Suárez a manipulator, Areilza said Suárez was trying to pull off "a democratic legitimation of francoism . . . to permit the continuation in power of the old ruling class." Areilza neglected to mention that he himself had been a prominent member of that class and longtime servant of Franco. In 1977 Areilza was one of the odd men out, and he didn't like it.

The UCD ran on a program of vague generalities: "to assure a more just distribution of wealth"; "the secure way to democracy"; "to impede polarization and establish a dialogue." Was the Centro really the political center? The Left said no, the "Centro" was on the Right, the moderate wing of neo-francoism, "last-minute democrats." This criticism was exaggerated. In contrast to the Alianza Popular, the Centro had no prominent francoist figures. Rather than francoists, *suaristas* were more plausibly modern conservatives; indeed, they even looked like well-pressed young Republicans. One might classify them as Center-Right—but change the hyphen into an arrow.

The opposition parties fumed at the built-in advantages of the Centro: Suárez's nonelected incumbency and his appearance in newspapers and especially

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on state-run television gave him unfair recognition. Motivated by pure opportunism, the Left charged, he was simply using the Centro to retain power. Suárez stood aloof, not campaigning personally and generally keeping Centro efforts low key. The UCD staged no mass rallies but tried to speak quietly to the silent majority. The strategy worked rather well.

SOCIALISTS

The second-place Socialists (*Partido Socialista Obrero Español*) are a close, perhaps conscious, approximation of the French Socialists of Mitterrand. Even PSOE's ubiquitous symbol—a rose in a clenched fist—was taken directly from the PSF.

PSOE bears the name of the grand old party of the Spanish Left. Founded by Pablo Iglesias in 1879, PSOE's present incarnation dates only from the 1960s when it was revived clandestinely. Some older Socialists, in exile in Toulouse, continued to claim the party's name and ran as a centrist party in 1977, PSOE (*histórico*). Again, jealousy was a factor, this time of young (thirty-five) Felipe González and his PSOE (*renovado*). Son of a leftist Andalusian cowherd, González joined the underground Socialists as a student. Working as a labor lawyer, González's intelligence and energy propelled him through PSOE ranks to become first secretary in 1974, a post he still holds.

PSOE's formula for success was also its weak spot. Its activists were young, often students, and they talked pretty radical: anticapitalism, anti-monarchy, anti-U.S. bases, etc. González was consistently moderate and at the party congress in December 1976 repeatedly beat back efforts to radicalize the PSOE program. But he could not do without the impassioned kids; he was forced to play a balancing act between them and the wider electorate. The result was a slightly schizophrenic PSOE in which González's vague and untheatening statements shared the podium with clenched-fist salutes and *La Internacional*.

PSOE in some instances assumed positions more radical than the Communists'. Formally PSOE was still "republican," and young militants often chanted at rallies: "*España, mañana, será republicana!*" No one sober wished the abolition of the monarchy at this delicate stage, and privately González was known to say he could live with it; he even met with the king. But González found it impossible to tell his militants to stop waving the republican flag (a striking red, gold, and purple) at rallies. On the stump, González tried to make the issue simply one "between change and continuism," that is, between himself and Suárez. The Centro and Alianza Popular were interchangeable with each other, he charged, the former "*franquismo renovado*" and the latter "*franquismo histórico*." In 1977 Spanish politics seemed to come in pairs, one histórico, the other renovado.

A plus for PSOE was its recognition as *the* socialist party of Spain by the Second International. The appearance on the stage with González of Brandt,

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Mitterrand, Nenni, and Palme served to assure voters that PSOE was in the mainstream of European democratic socialism. The German connection brought in abundant funds, allegedly millions of dollars from the DGB. PSOE's campaign was not badly financed; González even had a Lear jet to fly him to forty-five rallies in twenty-one days all around the country.

Despite a somewhat radical tone—depending on whether one listened to González or the kids—PSOE succeeded in capturing a goodly portion of the nonsocialist, middle-of-the-road voters who disliked the Franco regime and its inheritors. In 1977 PSOE began a process of broadening its base of support beyond committed socialists. Not yet a Center-Left party in 1977, PSOE had before it the choice of becoming one.

COMMUNISTS

The amazing Communists (*Partido Comunista de España*) took positions that the Socialists should have adopted. PCE accepted the monarchy (provided it was constitutional) and U.S. bases (to preserve East-West equilibrium) and said nothing about nationalization. Instead, the Communists hammered on one theme: the consolidation of democracy. The rest of their program was a modest socialist one.

How could Socialists and Communists seemingly switch positions in the political spectrum? The answer lies in the greater Communist discipline: members know they must obey the party line and do not need to be constantly fired with radical phrases. PSOE and PSP allowed themselves to be swept up by youthful enthusiasm, sometimes out of control. Paradoxically, greater freedom and pluralism in Socialist ranks propelled them leftward; greater control and centralism in Communist ranks permitted them to shift rightwards.

But closer up, PCE preserved some of its Stalinist past. Their smaller gatherings, attended largely by members, were redolent of "class struggle," "sharpening of contradictions," and "role of the artist in transforming society." Proclaimed one PCE candidate: "We haven't changed; we haven't renounced anything."

PCE Secretary General Santiago Carrillo at sixty-two was one of the oldest figures in the campaign. Son of a metalworker and PSOE leader, Carrillo shifted to the Communists in the mid-1930s, fought for the Republic, then went into exile in France until early 1976. Becoming party chief in 1956, Carrillo drew close to the Italian party line of Enrico Berlinguer to become a—perhaps the—leading exponent of Eurocommunism. In a shrewd move, the Suárez government authorized the return of Dolores Ibárruri, *La Pasionaria* of Civil War fame, now eighty-one, from Moscow where she had lived since 1939 as PCE's nominal president. The government sought both to underscore the party's Soviet ties and provoke internal conflict between pro- and anti-Moscow

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sympathies suspected of lurking within PCE. During the campaign, however, no such split emerged.

The Communist electoral effort was, as usual, superbly organized: more meetings, more posters, more show-biz personalities. But for Spaniards on the Left, PSOE and Felipe González were more attractive.

POPULAR ALLIANCE

The *Alianza Popular*, the only party on the Right to make it into the Congress, ran on a neofrancoist platform that stressed the achievements of the Franco era, its economic growth and stability, and this because there was order and a firm hand at the top. Founded in 1976, AP ran 172 members of the old Cortes for the new one, foolishly believing that their appointive status had conferred genuine popularity. Running in top positions for the Alianza were some of the leading cabinet ministers of the later Franco years, including Manuel Fraga Iribarne, AP's leader. A powerful personality who rose from modest origins, Fraga has been a political science professor and ambassador to London. Fraga was considered a liberalizer when he lifted direct press censorship in 1966 as minister of information and tourism. But Fraga's authoritarian streak became more pronounced with the years until, as interior minister in the second Arias cabinet (after Franco's death), he was a relative hard-liner on democratization.

Fraga denounced Suárez's reforms as a Trojan horse that would open the gates to leftist chaos. Observers suspect there was personal jealousy over the fact that a young unknown had been chosen to guide post-Franco affairs instead of the experienced Fraga. Suárez, in Fraga's eyes, was treacherous. He had taken an oath to uphold the Franco system (true) only to destroy it (also true). AP's electoral program, like most, sounded quite welfarish—housing, jobs, aid to the handicapped—but added Christian and nationalist themes. Mailings to priests, nuns, and military officers underscored that they and AP stood for the same things. AP's tag line in posters and ads was *"España, lo unico importante."*

Fraga, like González, sounded moderate on the campaign trail, sometimes mentioning Erhard's *Sozialmarktwirtschaft* as his model. But the word his audiences warmed to was "order." In the last weeks, AP's reasonable mask tended to drop away. *"Somos franquistas,"* loudly proclaimed at least one AP candidate (not Fraga), and the crowd roared approval. At Alianza rallies, chants of "Fraga, Fraga" shifted into "Franco, Franco."

The Alianza and Fraga personally were nearly everyone's bogeymen in the contest. At leftwing rallies the very name of Fraga brought wolflike howls and the chant, *"Fraga, el pueblo no te traga!"* (Fraga, the people can't swallow you!). AP was indelibly tarred with the image of repression and corruption, the rear guard of a cruel era. Its share of the vote indicated how little pro-Franco sentiment remained in Spain.

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POPULAR SOCIALISTS

The Popular Socialists (*Partido Socialista Popular*) were difficult to place in the political spectrum. Styling itself "Marxist and revolutionary," PSP on a 1–10 ideological scale fell off the paper. But at the same time, its leaders indicated they would be willing to serve in a Suárez cabinet! The schizophrenia that was latent in PSOE became manifest in PSP.

As the "Socialist Left," PSP denied the possibility of reforming capitalism. Before an overflow crowd at a Madrid bull ring—and a bull ring suited PSP perfectly—Candidate Miguel Alonso explained that PSP wanted "profound change, that the means of production pass into collective hands." In an unfraternal dig at PSOE and PCE, Alonso added: "We reject 'yellow' socialism and any historic compromise."

But party founder and President Enrique Tierno Galván, fifty-nine, said PSP could work with Suárez because the moment for socialism was not ripe: "The objective conditions for it don't exist." Tierno, an impish Marcusian figure, could criticize brilliantly but tended to shrug off practical questions with the confidence that "scientific socialism" can solve anything. His professorial abstractions gave PSP a utopian cast, and the party attracted little worker support; PSP was a party of radical intellectuals.

Tierno fought for the Republic and later became a law professor at Salamanca where he organized students into an embryonic socialist party. His political activity cost him his job, and he eked out a living tutoring students in his Madrid apartment. Many of Spain's younger professors, lawyers, and diplomats crammed for graduate exams under his guidance, a warm connection that lasts to this day.

Tierno, who had been patiently building a socialist party inside Spain since 1954, bristled at González's insistence that he simply dissolve his structure into PSOE. Jealousy again played a role here. A further blow to Tierno's pride came when the Second International supported PSOE and young González with endorsements and money. Tierno went looking elsewhere for funds and, it was widely believed in Madrid political circles, got a bundle from Libya's Kaddafi. Then, as if in revenge, PSP turned anti-Europe and pro-Third World (*tercermundismo*). PSP candidate Manuel Mella snarled: "What example are the German Social Democrats who govern a state that exploits migrant Spanish workers? What example are the English Labourites who maintain colonial situations like Gibraltar? What example is Israeli socialism which does not recognize the rights of the Palestinian people?" Hell, it seems, hath no fury like a socialist scorned. Such far-out rhetoric simply did not plug into an appreciable sector of Spanish society. Tierno and his party, sensing they were the odd men out on the Spanish Left, dissolved PSP in April 1978 and merged with PSOE.

CHRISTIAN DEMOCRATS

The surprise losers of the elections were the Christian Democrats (*Federación Democrata Cristiana*). They had recognized leaders, a head start in organizing,

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a good campaign, and foreign (especially Italian) endorsement. Yet they won not a single seat. The FDC was simply squeezed out of the political spectrum; between the Centro and PSOE there seems to be no room for an important party.

The Christian Democrats had two roots. One was José María Gil Robles, who during the Republic headed the rightist CEDA. In exile after the Civil War, he became a critic of Franco and exponent of conciliation. Too old to take a leading role in 1977, he stood aside in favor of his son, the bearded José María Gil Robles y Gil Delgado. Another root was inside the Franco regime. Joaquín Ruiz-Giménez, minister of education from 1951 to 1956, evolved from ultra-Catholic ambassador to the Vatican to a post-Vatican II Catholic progressive. Founding a study group and respected magazine, *Cuadernos para el Diálogo*, Ruiz-Giménez must be counted as one of the fathers of reborn Spanish democracy.

The Christian Democrats, like several other centrist parties, split, one faction going into the Centro, the other staying out. Father and son Gil Robles and Ruiz-Giménez, like Areilza, joined the ranks of the opposition. In contrast to the Center-Right positions of Italian and German Christian Democracy, the Spaniards were Center-Left. Said Ruiz-Giménez: "Christian Democracy is like the heart, in the center but toward the left." The FDC staked out a strongly welfarist program, almost as if they were social democrats, and took pains to make clear they had no special relationship with the Spanish Church.

What went wrong? First and foremost, the Christian Democrats were competing for the same electorate that was absorbed by the Centro, that is, middle-class Catholics not hostile to the post-Franco regime. Indeed, one survey found a rather neat tradeoff in most provinces: where the Centro was strong the Christian Democrats were weak, and vice versa.[4] Secondly, the Christian Democrats adopted a markedly socialist, sometimes almost Marxist, line that was far too radical for their natural constituency. FDC leaders had earlier drawn up a Christian Social program and stuck with it, not caring whether it cost them votes. A Christian Democratic party with a leftist program just does not wash: it loses votes from moderates and doesn't gain them from leftists. FDC's earlier survey percentages seem to have melted into the PSOE and Centro. The Christian Democrats were the odd party out in the political center.

Who Voted, How, and Why

In terms of the breakdown of the Spanish electorate, there should be no surprises for those acquainted with voting patterns in other European countries. According to a large (16,000-respondent) survey taken a week before the elections, the Centro emerged as the largest party in all social classes, but only narrowly in the lower class (Table 2). The Socialists drew nearly equal support

[4] J. Antonio Salgado, "La izquierda avanza," *Cuadernos para el Diálogo*, 4 June 1977, p. 43.

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TABLE 2

1977 Spanish Party Preferences by Class, Sex, Age, and Residence

		Social Class			Sex		Age			Residence[a]	
Party	*Total*	*Upper & U-Middle*	*Middle*	*Lower & L-Middle*	*Male*	*Female*	*21–34*	*35–54*	*55+*	*Rural*	*Urban*
Centro	29%	33%	33%	26%	25%	33%	24%	32%	30%	33%	21%
Socialist	23	11	23	25	27	19	32	22	16	18	26
Communist	7	4	6	8	10	4	10	6	5	4	11
Alianza Popular	8	22	9	5	8	8	4	8	11	7	8
Popular Socialist	4	8	6	3	5	4	7	4	3	2	10
Christian Democrat[b]	3	6	5	2	3	4	3	4	4	2	2
Other parties	7	10	7	5	9	4	9	5	5	3	9
Don't know	19	6	11	26	13	24	11	19	26	31	13
Total	100	100	100	100	100	100	100	100	100	100	100

Source: Madrid survey firm Sofemasa, "Analisis de los resultados obtenidos en el sondeo pre-electoral Junio 1977" (Xerox, undated). The author expresses his special thanks to Sofemasa director Gines Garrido Alart.

Note: Table is based on replies of those who said they were going to vote.

[a] Rural connotes places of fewer than 2,000 inhabitants, urban cities of more than 500,000.

[b] Including affiliated Basque Nationalists.

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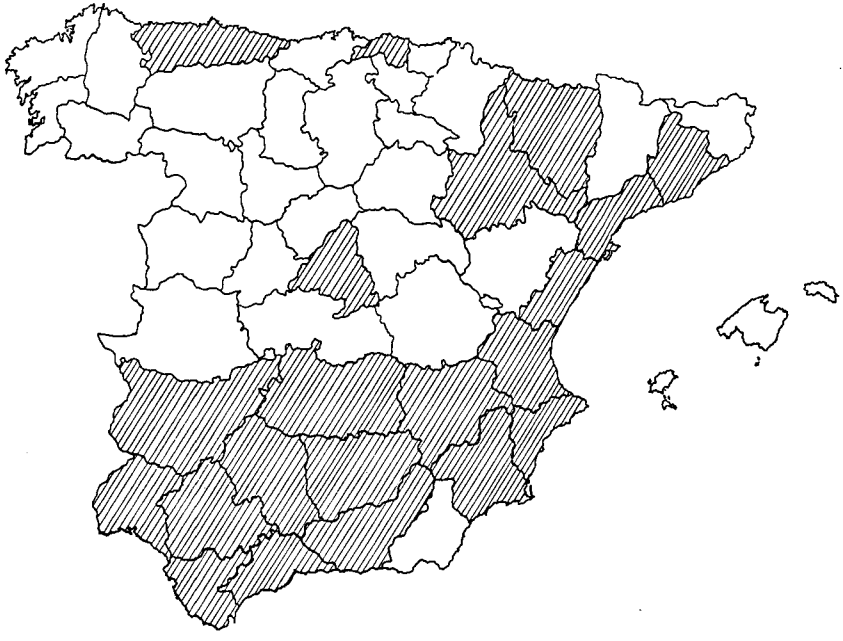
from the middle and lower divisions of this sample, underscoring PSOE's partly middle-class electorate. Accordingly, class-voting was rather weak, a pattern also typical of France and Italy. In contrast to some Northern European systems such as West Germany and Sweden, social class is a poor predictor of voting in Latin Europe. Again in the Latin European pattern, men vote more left than women, and women are more undecided. In Spain, as in many countries, the younger, the more left. One can see here why the Spanish Left parties vowed to lower the voting age to eighteen.

Marked regional patterns characterize Latin European polities. As can be seen in Map 1, this holds true for Spain. Just as France south of the Loire tends to go Socialist, so does Spain south of the Tagus. Spain's three largest cities, Madrid, Barcelona, and Bilbao, also went left. The best explanation for this pattern comes from Amando de Miguel's "six Spains" which he defined on the basis of regional social structure (Map 2). This Spanish sociologist, interestingly enough, was not looking for projected voting behavior when he constructed his divisions according to degree of proletarianization and size of the middle class.

MAP 1

Socialist Spain 1977



Shading indicates provinces with 40 percent or more total Left vote.

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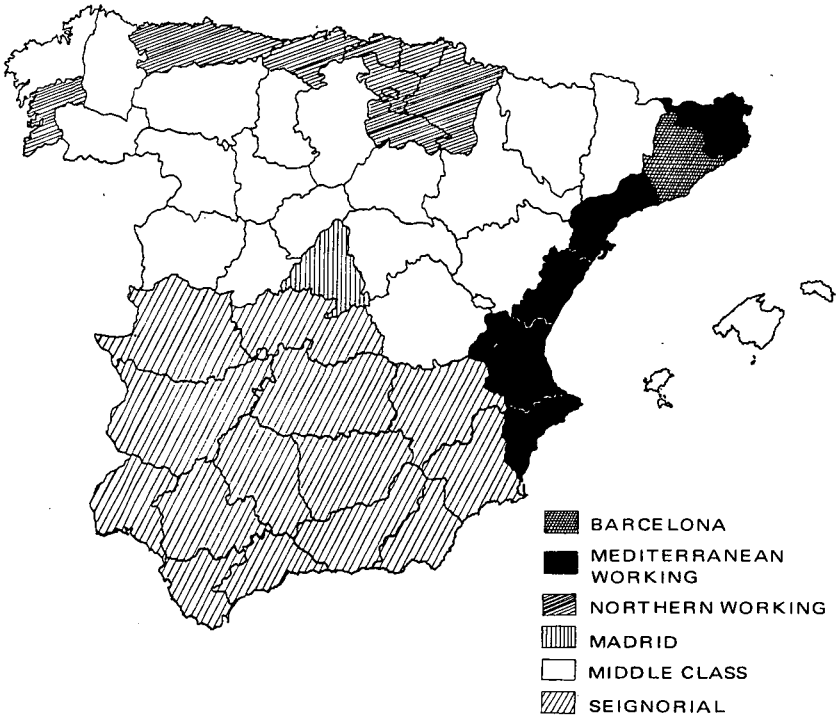
MAP 2

Amando de Miguel's "Six Spains"



Source: Amando de Miguel, *Recursos Humanos*, pp. 236–46.

The seignorial area is perhaps the most interesting, for it is precisely here, in Andalusia and Extremadura, that the Left did so well. Felipe González, a native of Seville, is the only Spanish political leader who looks and speaks like an Andalusian. As noted, the prevalence of large estates in the South tends to radicalize much of the local population, while owner-operated small farms in the North tend to produce conservative voters. A historical point also has some bearing. The areas initially seized by the Nationalists in 1936 went to the Right (Centro and Alianza Popular) in 1977 (Map 3). Two factors impinge: The long-established conservatism of these northern areas enabled local army garrisons to side with the Nationalist rebels in 1936. These provinces were relatively pro-Franco and were not penalized during or after the Civil War. Secondly, the areas the Nationalists had to conquer in the course of the Civil War suffered heavily both during and after the conflict. The 1977 balloting suggests this suffering was remembered and served to deepen existing leftist commitments.

What sort of public image did the parties and their leaders have in the

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MAP 3

Nationalist Spain 1936

Shading indicates areas seized by Nationalists in July 1936 at beginning of Civil War.

campaign? Some parties were perceived as more democratic than others. One poll found 36 percent saying PSOE would respect democratic liberties if it came to power; the Christian Democrats drew 26 percent, the Centro 21 percent, Alianza Popular 15 percent, and the Communists 14 percent. On the negative side, one-fourth thought AP and one-third thought the Communists would *not* respect democratic liberties.[5] PSOE's high score here suggests that some of its supporters may have been voting for democracy rather than socialism.

The 1977 elections were highly personalized. Pictures of all top party leaders—except the Communists' Carrillo—were featured on election posters. Voters had fairly clear likes and dislikes. In an early May survey, 68 percent said they "liked" Suárez and 42 percent González. The parties of the Left may have misplaced some of their energies in denouncing the most popular politician in the country, Suárez. At the top of the "don't like" list, the two leading AP figures, Fraga and Arias, were mentioned by 53 and 51 percent, respectively.[6]

[5] José Félix Tezanos, "La imagen de los partidos," *Cuadernos para el Diálogo*, 21 May 1977, p. 41.

[6] Joaquín Prieto, "Suárez, el político más popular, seguido, a distancia, por Felipe González," *El País*, 11 May 1977, p. 12.

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Thus the key personalities of Alianza Popular were the most unpopular politicians in Spain, a severe obstacle in any election.

Top national leaders were far better recognized than local candidates. For example, 5.3 percent of respondents in Galicia said they would vote for Carrillo (who wasn't running there), while a mere 0.3 percent said they'd vote for the local Communist leader. Likewise in Catalonia, 8.1 percent said they'd vote for Fraga (who was running in Madrid), while local candidate ex-Miniter Lopez Rodo drew only 0.9 percent. In Andalusia, native son Felipe González drew 23.8 percent support while the local PSOE candidate got only 0.9 percent.[7] The contest thus became more like those associated with presidential systems. A good rally was one where the national leader appeared; purely local figures were used to fill up the time.

Madrid was the prestige district to compete in, and all the top figures ran there as the first names on their parties' lists for Congress. Although the Madrid district is not representative of Spain—it's to the left of the nation as a whole—leaders took the race there as a personalized *mano a mano*. It was particularly important for Suárez, in his effort to hold the Centro together after the elections, to do well in that traditionally Socialist city, and he did:

Party	*Leader*	*Percent*	*Seats*
Centro	Suárez	32.2	11
PSOE	González	31.5	11
Communist	Carrillo	10.7	4
Alianza	Fraga	10.5	3
PSP	Tierno	9.1	3
FDC	Ruiz-Giménez	1.5	0

The Disjunction Problem

Several observers of the 1977 elections held the view that the Spanish Left parties had taken positions too radical for their prospective electorates. This may be termed a "disjunction": extremist politicians out of touch with moderate voters. With the Left politicians too far left for their normal constituents, some charged, a gap opened up in the Spanish political spectrum where there should have been a Center-Left party. Disjunction is an apt description of the problems of at least two parties, the Christian Democrats and Popular Socialists. The PSOE seemed at first to be also too far left, but by the finish was appealing to many centrist Spanish voters. The "gap at Center-Left," however, does not hold up; there was no gap in the Spanish political spectrum.

That the Spanish people were ideologically moderate is clear. A summer 1976 survey by the reliable Madrid organization DATA asked over 6,000 Spaniards to place themselves on an ideological scale, 1 for the most Left, 10 for the most Right. They averaged 5.64, putting them between Belgians (who averaged 5.67) and Germans (5.63). Indeed, 31 percent of the Spaniards

[7] "Ningún partido monopoliza una clase social," *El País*, 28 May 1977, p. 13.

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placed themselves at 5 and 18 percent at 6.[8] The election results confirm the survey's accuracy: the parties that appealed to moderates did well; others shriveled.

But would a different, more moderate, political line by the Left parties have increased their share of the vote? The election results suggest not. In most West European democracies the total Left vote hovers between 45 and 50 percent. The total Spanish Left vote in 1977 was 45 percent; the Spanish Left failed to win not more than a couple of percentage points at most. (In 1936 the Left won 47 percent.) In running against the most popular politician in the country, Suárez, the Spanish Left did quite well. A more moderate line would probably not have made a dent in the Centro vote.

The charge that the Left's rhetorical excesses produced a gap where there should be a Center-Left party is likewise hard to demonstrate. In addition to the ill-fated Christian Democrats there was a small Center-Left grouping, the *Alianza Socialista Democrata*, consisting of the PSOE (*histórico*) and Partido Socialista Democrático Español. They even advertised themselves as the *Centro Izquierda*, non-Marxist and anticommunist. They won a small fraction of one percent and no seats.

Little support for the disjunction view can be adduced from the high proportion of those surveyed who could not, before the election, state a party preference. One would expect many undecideds in a first-time election. Without detailed survey data at our disposal, it would seem that most of the undecideds, perhaps over two-thirds, finally went to the Centro or PSOE in about equal proportions (Table 3).

TABLE 3

Evolution of Party Preferences, May-June 1977

	May 4–5 Survey	*May 18–19 Survey*	*June 6–8 Survey*	*June 15 Election*
Centro	22.3%	23.6%	30.2%	34.7%
Socialist	14.5	15.8	24.0	29.2
Communist	5.9	6.8	7.2	9.2
Alianza Popular	5.3	6.7	8.2	8.4
Popular Socialist	6.0	4.5	4.8	4.5
Christian Democrats	1.2	3.5	2.3	1.4
Other	11.6	9.0	12.1	12.6
Undecided	33.3	30.1	11.2	–

Source: *El País*, 24 May 1977, p. 1, 12 June 1977, p. 1, and 17 July 1977, p. 1.

The two parties that failed to grow in the course of the campaign, the Christian Democrats and PSP, were respectively sunk by and stunted by the disjunction problem. A similar fate might have befallen PSOE, but a couple of

[8] "El electorado español, un electorado europeo," *Opinión*, 8 January 1977, p. 28.

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factors intervened. The big PSOE rallies (many with over 10,000 attending) conveyed an impression of maximalist socialism. Fortunately for PSOE, most voters did not go to these rallies, and news reports concentrated on González's moderate statements on free public education, national medical care, etc. PSOE's net public image, therefore, was less radical than a closeup image. More important, however, as the campaign neared its close, PSOE emerged as the only game in town for otherwise moderate voters who disliked francoism and saw Suárez as its inheritor. The question PSOE should ponder is how many nonsocialist voters did it gain by virtue of being a *democratic* option? The rapid growth of PSOE supporters in Table 3 suggests that toward the close it was drawing previously uncommitted democrats rather than firm socialists. If PSOE realizes this, Spain may yet have a large Center-Left party and with it a reasonably stable political system.

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Origins of the Cold War: Some Clues from Public Opinion

GEORGE H. QUESTER

The aftermath of the Vietnam debacle saw a substantial reexamination of the entire cold war, with some analysts arguing that most American policies since 1945 have been just as mistaken and needlessly combative toward Communist-governed nations as the intervention in Vietnam.[1] Not so surprisingly, persons who participated in the development of these policies have responded that the policies were not mistaken or foolish at all, but a sensible response to a real threat, or to an inherently combative situation.[2]

Some of this is, of course, the fighting-out of current debates by the use of historical surrogates. Many might find it logical that to dismiss the Communist

[1] There is no shortage now of literature on the cold war's origins. Two very valuable collections of articles and chapters on the question are Charles Gati, ed., *Caging the Bear* (New York: Bobbs-Merrill, 1974), and James V. Compton, *America and the Origins of the Cold War* (Boston: Houghton Mifflin, 1972).

Interesting examples of one form or another of the "revisionist" argument can be found in Gar Alperovitz, *Atomic Diplomacy: Hiroshima and Potsdam* (New York: Simon and Schuster, 1965) and *Cold War Essays* (New York: Anchor Books, 1970); Ronald Steel, *Pax Americana* (New York: Viking, 1967); N.D. Houghton, ed., *Struggle Against History* (New York: Simon and Schuster, 1968); Richard Barnet and Marcus Raskin, *After Twenty Years* (New York: Random House, 1965); Gabriel Kolko and Joyce Kolko, *The Limits of Power* (New York: Harper & Row, 1972); Frederick Schuman, *The Cold War: Retrospect and Prospect* (Baton Rouge: Louisiana State University Press, 1967); Carl Oglesby and Richard Schaull, *Containment and Change* (New York: Macmillan, 1967); and David Horowitz, *The Free World Colossus* (New York: Hill and Wang, 1965); Barton Bernstein, ed., *The Atomic Bomb: The Critical Issues* (Boston: Little, Brown, 1976); Martin Sherwin, *A World Destroyed* (New York: Alfred A. Knopf, 1975).

[2] For analyses along more "conventional" lines, see: Graebner, *Cold War Diplomacy* (Princeton: Van Nostrand-Reinhold, 1962); G.F. Hudson, *The Hard and Bitter Peace* (New York: Praeger,

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military threat today would mean to dismiss it as well in 1946 or 1953, while to believe in it for the earlier years would be to question the possibility of détente today. At least one other possibility may have to hover in the background of our discussion, however, that the threats of political and military instability were greater and more real in the late 1940s than they are in the 1970s, such that a "consistency" of interpretation over time may not be in order at all. If resistance to the Communist takeover of Vietnam was a mistake, resistance to the Berlin blockade may not have been, etc.

Leaving aside for the moment this *ad hominem* interpretation of the debate (that the events of 1945 are too much interpreted in terms of the issues of 1975), is there some underexploited kind of evidence which might tell us more about the origins of the cold war? The premise of this article is that there is such evidence, namely in the public opinion poll data of the time, shedding light on how the man on the street saw the cold war as it began, perhaps rendering a very different picture from what a student or a man on the street would assume today.

Claims vs. Performance

If one felt there was an "objective" answer to the question of "who started the cold war," how could one indeed settle the issue? Some defenders of American policy would find support in the record of straightforward self-description. Aggressors and imperialists in the past have often admitted to being so, and it is easy enough to find aggressive outbursts in the private conversations and public speeches of Adolf Hitler. By deductive processes which are more debatable, one could find expansionist self-descriptions in Communist statements about the future of the international system.

By contrast, where are the American statements of aggressive intent? If there were relatively few expressions of desire for territorial aggrandizement in the United States as World War II ended, would this not prove that the guilt for the cold war lay elsewhere? Consider, for example, this array of options on which the American public was polled from 1942 to 1946:

U.S. 1942–1946[3]

Which one of these four statements comes closest to what you think the United States should do about new land and possessions in the world after the war?

1967); Paul Y. Hammond, *The Cold War Years* (New York: Harcourt Brace Janovitch, 1969); Lisle Rose, *Dubious Victory* (Kent, Ohio: Kent State University Press, 1974); Adam Ulam, *The Rivals* (New York: Viking Press, 1971); Herbert Feis, *The Atomic Bomb and the End of World War II* (Princeton: Princeton University Press, 1966); and *From Trust to Terror: The Onset of the Cold War, 1945–1950* (New York: W.W. Norton, 1970); Seyom Brown, *The Faces of Power* (New York: Columbia University Press, 1968); Robert James Maddox, *The New Left and the Origins of the Cold War* (Princeton: Princeton University Press, 1973); and John Lewis Gaddis, *The United States and the Origins of the Cold War, 1941-1947* (New York: Columbia University Press, 1972).

[3] Quoted in Hadley Cantril, *Public Opinion, 1935–46* (Princeton: Princeton University Press, 1951), p. 135.

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	Feb. '42	June '43	Mar. '45	Aug. '45	Feb. '46
Give up all the land outside the U.S. that is difficult to defend	10%	9%	7%	4%	9%
Be satisfied with the amount of land in the world she had before she entered war	42	37	28	17	26
Try to get new military bases, but nothing else	34	44	53	60	55
Try to get as much new land in the world as she can	14	10	12	19	10

That the American public would now want some additional military bases is not really surprising, given all the World War II regrets about the pre-1939 geographical stance of isolation; the question wording makes clear that the "new" military bases are as compared with "the amount of land in the world (the U.S.) had before she entered the war." But the more explicit expansionist statement by contrast does quite badly.

Yet this of course raises problems of self-delusion and internal propaganda brainwashing. So what if Americans insist on telling themselves that they are not expansionist? Aren't Americans by the very nature of the capitalist system prone to convincing themselves that they have only defensive and altruistic motives, while actually needing to be imperialistic and expansionist for a host of economic and psychological and political reasons? In the process of acquiring economic investments along with the military bases, couldn't Americans really have been getting all the control they wanted, without getting into any formal and acknowledged sovereignty over "new lands and possessions"?

An alternative yardstick of blame for the cold war might instead base itself on the results of the fray. It is thus fashionable for young Americans today to revise history by suggesting that only the United States was expansionist from 1945 to 1965, or, if both the Soviet Union and the United States were expansionist in intention, that only the United States was successful in this regard. The USSR from 1949 to 1965 succeeded in winning control over Cuba, and lost control over China, a poor balance sheet indeed. In the same years, American influence replaced that of Britain from the Middle East to Malaysia to Australia, for a time replaced that of France in Vietnam, and established itself in scores of other locations around the globe. It might seem that success at expansion is prima facie evidence of propensity for expansion or desire for expansion. After some thirty years of often hostile interaction, who thus expanded the most?

A defender of the U.S. foreign policy could pick some examples from recent history to cast doubt on this as any straightforward yardstick. Were there any less successful countries than Germany or Japan in terms of expansion? Is their lack of success thus to be proof of their lack of interest in territorial conquest in the 1930s? American and British troops are now based deep in Germany. Does this prove that London and Washington were more expansionist regimes than Berlin in the 1930s? Such counter examples thus obviously suggest a model of

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regimes defensively moving their troops forward to forestall expansions, of status-quo-minded regimes wiping out or containing a threat to the status quo. Would not many or most Americans have described their own motivations in the same way for the cold war? If America thrust its influence into more previously uninfluenced square miles than did China or the Soviet Union, this might not per se demonstrate a greater taste for such influence in Washington than in Peking or Moscow.

Critics of American foreign policy might come back to suggest that the fascist states and the United States have been all too parallel, in that each ceased to implement its expansionist intentions only after it had been beaten in war, the Germans and Japanese in 1945, the United States in Vietnam. Defenders of American policy will claim that it has instead been the Communist countries whose expansionism has been blocked, in Korea and Berlin in the 1940s and '50s, in Portugal in the 1970s, etc.

One upshot of this at least might be that it is so easy to prove aggressive desires by outcomes. Just as the establishment's "self-descriptions as proof of intentions" should be discounted, so also have to be the revisionists' "results as proof of intentions."

"Neutral Witnesses"

A third, more satisfying, yardstick might of course be bystander judgment, if it can be found. Whom would Swedes have seen as more expansionistic in 1938, Germany or Britain? Whom would they have seen as more expansionist in 1946, the United States or the Soviet Union? The answer to the latter question might come as a surprise to Americans who by now are used to being denounced from Stockholm for ventures in Vietnam or Chile. Tables 1 and 2 show that Swedes in 1946 and 1948 trusted the United States much more than the Soviet Union:

TABLE 1

Swedish Opinion, March 1946

Do you think that one or more of the great nations should limit their own claims for the sake of a more peaceful cooperation in the world?

Russia	30%
USA	4%
England	5%
France	1%
All nations	41%
No nation	3%
Don't know	23%

Source: Hadley Cantril, *Public Opion, 1935–46* (Princeton: Princeton University Press, 1951), p. 788.

Note: Percentages add to more than 100 because some respondents gave more than one answer.

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TABLE 2

Opinion in Eight Nations, February 1948

Do you think Russia (the United States) would start a war to get something she wanted (such as more territory or more resources)—or would she fight only if attacked?

	Would Start War	Defense Only	No Opinion
Russia:			
United States	73%	19%	8%
Canada	60	26	14
Holland	57	27	16
France	51	22	27
Italy	50	17	33
Brazil	43	16	41
Sweden	42	21	37
Norway	37	37	26
United States:			
Norway	23%	55%	24%
France	20	56	24
Holland	16	60	24
Italy	16	48	36
Canada	13	77	10
Sweden	13	54	33
Brazil	9	53	38
United States	5	92	3

Source: George H. Gallup, *The Gallup Polls* (New York: Random House, 1972), vol. 1, p. 709.

Similar judgments show up again and again across Europe. One notes with interest some Italian polls of 1953 and 1963, which found the United States *almost* as much distrusted as the USSR at the earlier year, but much more to be trusted than the USSR ten years later:

Italy 1953, 1963[4]

Some people say that America (Russia) wants to dominate the world. What do you think about that?

	True	Partly True	False	Don't Know No Answer
America:				
1953	29	40	23	8
1963	15	21	41	23
Russia:				
1953	47	22	15	16
1963	38	19	22	21

Opinion polls rarely get taken in the Communist-governed countries, so that the net total of poll data is thus skewed toward densest coverage of the "free

[4] Published in *Polls*, vol. I, no. 1, (Spring 1965): 40.

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world," as all but the Swedish polls are from countries allied with the United States. Perhaps even the Swedish findings should be dismissed, as Nikita Khrushchev once dismissed Dag Hammarskjöld by stating that "there are no neutral men." The Swedes of 1946 and 1948 were still surely enough part of the capitalist system to be accused of being blinded to its imperialist proclivities; they were also still enough concerned about the particular vulnerability of the buffer in Finland to be fixated on Russia as the paramount military menace.

Perhaps. Yet the critic of American foreign policy cannot with total comfort cite Swedish opinion in 1968 as evidence on the folly of the continuation of the cold war in Vietnam, and totally dismiss such evidence on the cold war in 1948. The opinion we can find—outside the United States—on the blame for the "outbreak of the cold war" is not ambivalent; it blames the Soviet Union much more than the United States.

The retrospective issue of the foolishness or wisdom of American engagements in the cold war moreover does not simply hinge on the issue of *blame*. We will come shortly to some "blameless" models of international conflict, models in which military preparations are still necessary and wise, in which political confrontations may be inevitable. Even if the Swedes have not been neutral in the struggle between socialism and capitalism, they can be treated as sensible commentators on whether a need for confrontation existed; if their judgment in 1968 was against confrontation with the Communist world, in 1948 it converged more on supporting it.

"Facts" in an Atmosphere of Mistrust

A fourth approach to answering the question of "who started the cold war" might instead try to go to the history itself, rather than our neutral or nonneutral opinion data, in hopes of establishing the chronological sequence of who made the first hostile move. But there are problems here too. This approach can overlook the possibility of "overreaction," where a small insult or provocation on one side is followed by enormous and more noticeable expansions or aggressions on the other. It also then invites an infinite recursion into the past, as each "initial provocation" on one side is rebutted by a "prior hostile move" newly uncovered on the other side, going back further into time. This approach, in short, constitutes a very attractive invitation for historians to redo and revise a great deal of past research, but it does not promise to resolve the core questions.

It is at least possible that there is no answer to the question of "who started the cold war." Political scientists familiar with the game-theoretical concepts of prisoners' dilemma will see how an environment may drive both sides simultaneously into double-crossing each other. A major change in the international system, such as the final collapse of Nazi Germany, may thus instantaneously have converted two previously nonhostile positions into positions of threat and menace in each direction, thus generating two simultaneous "first hostile moves."

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One conclusion to this discussion might thus come down simply to a plea for greater agnosticism on the "fault" of the cold war. Small missteps can trigger opposing missteps slightly greater in magnitude, and so on, and so on. Who began the alienation between Stalin and Truman may always be a little unknowable, along with who began the alienation between Nixon and Allende, between Sadat and Khaddafy, etc.

Yet this will not satisfy everyone, for there are at least some occasions when such agnosticism will be quite inappropriate. Hitler attacked Denmark in 1940; Denmark had not attacked Nazi Germany. If we really get at the facts, by painstaking research, will the true origins of the cold war between the United States and the Soviet Union not become equally clear?

U.S. government criticisms have been based most heavily on Soviet failure to abide by promises and commitments. The American stress on Russian treachery here may in part reflect the traumas of Pearl Harbor, and memories of Hitler's failures to abide by promises made at Munich. It may also have served as effective propaganda with which to mobilize support for "eternal vigilance" against the Soviet Union.

Yet it is altogether remarkable how the opinions of nation after nation in 1957 shared the view that the Russians could not be counted upon to keep an important promise (a feeling which indeed rose in each case with education). Despite our disagreements (or agnosticism) about this in the present, the case seemed clearer then than we might have remembered it:

Thirteen Nations, October 1957[5]

If we did accept the proposal (to suspend H-bomb tests for two years), do you think the Russians would keep their promise to stop their own H-bomb explosions?

	Yes	No	Don't Know
Netherlands	13%	65%	22%
Australia	21	61	18
Sweden	16	59	25
Britain	27	56	17
Denmark	12	54	34
Germany	19	53	28
Austria	23	47	30
Belgium	19	44	37
Italy	23	43	34
Norway	24	40	36
France	24	38	38
Brazil	29	33	38
Japan	23	32	45

[5] Polls compiled and published in Helen Gaudet Erskine, "The Polls: Atomic Weapons and Nuclear Energy," *Public Opinion Quarterly* 27 (Summer 1963): 170.

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	Percentages Who Feel Russia Would Not Keep Promise	
	Primary School or Less	Secondary School or More
Austria	43%	52%
Belgium	39	48
Denmark	51	66
Germany	42	57
Italy	34	59
Japan	28	39

One still has the problem of course, that no polls are available from the Socialist bloc to even the count. (Yet what would a fairly taken poll of Russian reliability have shown in Warsaw, after the Russian delay in coming to aid the uprising of 1944, or in Budapest after the Russian "withdrawal" and reappearance in 1956?)

The policy decisions of the two sides responding to the end of World War II thus inevitably served to obscure the facts. Each side may have made its moves on the basis of cautious preliminary predictions about the other, and in doing so seemed to confirm the other side's more pessimistic predictions, and thus forever blurred the historical record. The Heisenberg Uncertainty Principle may be even more applicable to politics than to physics. What can expressions of opinion then show, if the facts of the sequences of the two sides' cold war policy choices remain debatable?

A few would thus leap to claim that we can again get to the issue of cold war blame, and (on the basis of data cited above) restore that blame to the Soviet side despite the revisionist arguments. A more modest conclusion would be that such widespread expressions of mistrust at least show how the international situation from 1945 to 1960 made tension inevitable in all directions, and therefore made cautious policies sensible. The cold war may indeed not have been folly, but may have been the necessary result of a global situation which drove the powers to mutual confrontation. If nature has been more kind to us since 1960 or 1965, this does not demean the wisdom of those making policy before then.

"Expansion" or "Containment"

What then was the nature of American foreign policy attitudes as the cold war reached its peak? Was it simply a response to the threat of Soviet expansionism, as Americans told themselves? Or was it also consciously expansionist, as the United States sought to win additional markets and military bases and "hearts and minds"? Or was it quite unconsciously expansionist, with all the paradoxes this presents?

If the United States was expanding in the postwar years in a manner not noticeable in Washington or London, this might yet seem comparable to some menacing empires of the past. Historical analogy might be drawn with the ex-

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pansion of British naval power into all the oceans of the world in the eighteenth century, after having been primarily protective in its original intent; designed simply to keep the Spanish and French from crossing the English Channel, the Royal Navy was after a time to practice gunboat diplomacy around the globe. Perhaps German growth in the years prior to World War I similarly amounted to "unconscious imperialism," an unwitting accretion of power that after a time was to become menacing to other states.

Yet the British had sung "Rule Britannia, Britannia rules the waves," while Americans by and large never went past talking of containment. A subtle second-order difference, perhaps, but a difference nonetheless.

Perhaps the very best illustration of everyone's perceptual problem on comparative expansionism shows up in this phrase "containment." Americans and others might find it perfectly reasonable that the Soviet Union not be allowed to expand the area it controlled, that the Soviet bloc thus be "contained." Yet this formulation tended to fixate most attention on keeping the Russians north of the Iranian border, and thus diverted thought from what was happening in Iran, as long as the Russians did not penetrate. And similarly in Southeast Asia, and Africa and everywhere else. But what if the United States were thus indeed moving into all these territories as part of the process of buffering the barriers to Soviet expansion, moving in to replace Britain or France or other powers? Would the indictment not soon enough have to be confirmed that the United States was proved to be more expansionist than the Soviet Union?

But problems of gestalt and image and axiomatic understanding again arise here. By some traditional standards of legitimacy, the American presence did not constitute aggression or conquest. American airbases in Morocco had been freely granted by France, and American advisers in Malaya had been invited in by the British. Americans would thus not see themselves as antistatus quo in all these regions, but merely more active partners in a common effort at resisting aggression, at preventing the overturn of a legitimate status quo. As we have seen, moreover, this was for many years the image of the American role found most plausible in European countries also.

One of the most revealing indicators of self-image, and comparative sense of expansionism, through the years of confrontation comes with opinion on "who is winning the cold war." If Americans were consciously relishing the task of occupying positions previously French or British or Japanese, one might have expected more sanguine reports of "victory" here. Yet the polls as taken in the United States repeatedly show an impression of the Russians "winning," that is, doing better in the sweepstakes of national power and prospective territorial control.

United States, 1948–1951[6]

In general, who do you think is winning the cold war—Russia or the United States?

[6] Published in Helen Gaudet Erskine, "The Cold War: Report From the Polls," *Public Opinion Quarterly* 28 (Summer 1961): 302.

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Asked only of those who correctly answered: Will you tell me what the term "cold war" means?

	Russia	U.S.	Neither	No Opinion	Unfamiliar with "Cold War"
December 1948	15%	17%	16%	6%	46%
March 1950	23	16	5	14	42
January 1951	30	9	12	4	45

United States, 1958[7]

Which country—the United States or Russia—do you think is farther ahead in the field of long range missiles and rockets (education) (living standard) (propaganda)?

	Entire Sample		College-trained	
	U.S.	Russia	U.S.	Russia
Missiles	37%	40%	32%	48%
Education	68	18	64	25
Living Standard	94	1	97	*
Propaganda	44	38	31	54

*Less than 1%

Americans, in particular college-trained Americans, here typically saw the United States ahead in the attributes that *merited* a defense, education and standard of living, but behind in the attributes that *facilitated* a defense, weapons and propaganda. Americans may thus have been unconsciously expansionist in these years, but one would have to put at least as much stress on the "unconscious" as on the "expansionist." The erection of an American defense line in Iran or Greece or Malaya may not have been seen as so much a plus for the United States, but as the prevention of a minus for "the West," not so much as expansion, but as defense or containment.

The Slow Emergence of the Cold War

Quite apart from the issue of who, wittingly or unwittingly, is to blame for starting the cold war, we might now question the conventional assumption that this confrontation was instantly to appear after World War II. At the level of public reactions, 1946 and 1947 show almost the same ambivalence toward a monolithic communist threat as 1970. Winston Churchill's 1946 Fulton, Missouri, speech is retrospectively remembered as the clarion call for resistance to Russian expansionism, but at the time it drew much more American criticism than praise:

United States, April 1946[8]

When the country was questioned about Churchill's suggestion that a partnership

[7] Published in George H. Gallup, *The Gallup Polls* (New York: Random House, 1972), vol. II, p. 1571.

[8] Published in "The Quarter's Polls," *Public Opinion Quarterly* 10 (Summer 1946): 264.

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between the United States and Britain should be formed to act as a check on Russian moves, 68 percent of the people said they had heard about or read his speech.

What is your opinion on his suggestion? Asked of the 68 percent who knew about the speech.

Approved his idea, thought it was right	18%
U.S. and Britain should stick together, but have no military alliance	4
Disapproved of Churchill's suggestion	40
No opinion	6
	68%

Even a year earlier, the Yalta Agreement, which afterwards was painted as so much a triumph of Soviet cunning over American naivete, had won overwhelming endorsement:

United States, March 1945[9]

Have you heard or read about the Crimean Conference between Stalin, Churchill, and Roosevelt?

Yes	70%
No	30

Those who had heard or read about the Crimean Conference were then asked: On the whole, is your opinion of what was accomplished at the conference favorable or unfavorable?

Favorable	61%
Unfavorable	9
No opinion	30

While it is sometimes contended that the American use of the atomic bomb against Hiroshima and Nagasaki was intended to end the war before the Soviet Union could declare war against Japan, the popular sentiment of the time overwhelmingly favored the idea of Russian entry, with fears expressed rather widely that the Russians would choose not to enter the war:

United States, June 1945[10]

Do you want to see Russia join us in the war against Japan, or would you rather not see her join us?

Want Russia to join	77%
Do not want	14
No opinion	9

Do you think Russia will join us in the war against Japan?

Yes	39%
No	31
No opinion	30

[9] *Gallup Polls*, vol. I, p. 492.
[10] Ibid., vol. I, p. 515.

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TABLE 3

American Opinion Toward Russia, 1942–49

Do you think Russia can be trusted to cooperate with us after the war is over?					*Yes*	*No*	*Undecided; no opinion; don't know; no answer*
NORC	Feb	14	'42		38%	37%	25%
OPOR	Mar	26	'42		39	39	22
*NORC	May	6	'42		45	25	30
OPOR	June	17	'42		41	33	26
*NORC	July	1	'42		45	26	29
OPOR	July	15	'42		50	30	20
*NORC	July	18	'42		43	27	30
*NORC	Aug	21	'42		51	25	24
*NORC	Nov	19	'42		51	24	25
*NORC	Nov	27	'42		52	26	22
AIPO	Jan	7	'43		46	29	25
AIPO	Apr	6	'43		44	34	22
*NORC	June	18	'43		48	27	25
AIPO	Nov	10	'43		47	27	26
*NORC	Nov	15	'43		48	20	32
AIPO	Dec	15	'43		51	27	22
AIPO	Jan	18	'44		40	37	23
*NORC	Apr	8	'44		50	22	28
AIPO	June	7	'44		47	36	17
**AIPO	Nov	15	'44		47	35	18
**AIPO	Feb	20	'45		55	31	14
**AIPO	May	15	'45		45	38	17
**AIPO	Aug	8	'45		54	30	16
**AIPO	Oct	17	'45	first cross-section	38	45	17
†AIPO	Oct	17	'45	second cross-section	44	40	16
†AIPO	Feb	27	'46		35	52	13
†AIPO	Apr	10	'46		45	38	17
†AIPO	Sept	25	'46		32	53	15
†AIPO	Dec	11	'46		43	40	17
†AIPO	June	26	'49		20	62	18

*The question was: Do you think Russia can be depended upon to cooperate with us after the war?

†Do you think Russia will cooperate with us in world affairs?

**Do you think Russia can be trusted to cooperate with us after the war?

Sources: Hadley Cantril, *Public Opinion, 1935–46* (Princeton: Princeton University Press, 1951), pp. 370–71; "The Quarter's Polls," *Public Opinion Quarterly* 13 (Fall 1949):550.

The last poll question of course was posed when the Americans interviewed could not know of the atomic bomb, while the Truman decision makers did. It is still arguable therefore that the privy American elites wanted to keep the Soviet Union out of the war, even if an overwhelming fraction of ordinary Americans wanted the Russians in. In a revisionist view, therefore, the "outbreak of the cold war" at the leadership level indeed could have come in 1945,

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with the Truman administration laboriously training a reluctant public to join in this confrontation, a training which would only be completed by 1948 or 1950.

If one chose neither to accept nor to reject this picture for the moment, however, the evidence from poll data at least cautions against our misremembering any instant results at getting Americans cold-war-minded. It was to be some time yet before a president who wanted to get Americans to feel more in conflict with Moscow would thereby be improving his elections prospects. If the man-on-the-street had his doubts about Russian reliability, he also apparently had some doubts about Britain also.

If America was burdened by a capitalist outlook or unadmitted expansionism, it nonetheless did not yet really see itself so totally engaged, at least until some events passed to generate what might be interpreted as evidence of Soviet intentions. The evidence came slowly. The aftermath of World War II in many ways set up the situations which would tempt Washington and Moscow to race each other to the political prizes, but the war had also wearied each nation enough to require some time for recovery. In polls taken as late as a few months before the outbreak of the war in Korea, one thus finds many Americans, almost a majority, surprisingly unable to define what was meant by the phrase cold war:

United States, February 1950[11]

Will you tell me what the term "cold war" means?

Correct answer	58%
Incorrect, don't know	42

For purposes of tracing cold war sentiment, we are blessed here with two questions (Tables 3 and 4) which were posed repeatedly in opinion surveys, questions which replace each other as events seemed to dictate a change, but which can be almost coupled and laid end to end to keep hold of the trends. The first, from 1942 to 1949, asks "Do you think Russia can be trusted to cooperate with us. . ." The second, from 1946 to 1953 asks whether "you believe that Russia is trying to build herself up to be the ruling power of the world. . ."

Several generalizations seem justified on the basis of these polls. The "cold war" was not in full bloom until something happened in 1947 and 1948 to raise American distrust of the Soviet Union to a new plateau. Something else then happened in 1950 to raise it to yet another plateau. Were these "happenings" breakthroughs in the American government's brainwashing effort? It becomes almost an exercise in epistemology to argue that issue on either side. Or were the happenings in the actual events of the Soviet-American confrontation?

1947–1948

When we turn to polls on specific problems, we may get a finer focus on events here. Polls taken about American aid to Greece and Turkey in 1947 thus display

[11] Ibid., vol. II, p. 918.

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TABLE 4

American Opinion Toward Russia, 1946–53

As you hear and read about Russia these days, do you believe Russia is trying to build herself up to be the ruling power of the world, or is Russia just building up protection against being attacked in another war?

	Ruling power	*Protection*	*No opinion*
1946 June	58%	29%	13%
1946 July	60	26	14
1947 October	76	18	6
1948 March	77	12	11
1949 May	66	15	19
1950 January	70	18	12
1950 November	81	9	10
1953 July	79	10	11

Sources: George H. Gallup, *The Gallup Polls* (New York: Random House, 1972), vol. I, pp. 581–82, 682, 721; vol. II, pp. 881, 949, 1163–64; "The Quarter's Polls," *Public Opinion Quarterly* 14 (Spring 1950): 192.

considerable uncertainty about the desirability of such aid, with expressions of concern that the moves will be provocative to the Soviet Union, possibly causing an unnecessary war:

United States, March 1947[12]

Do you think we should or should not send some American military experts to Greece to improve their armed forces?	Should	47%
	Should not	41
	Don't know	12
Do you think we should or should not provide military supplies and some American military experts to Turkey to strengthen her defenses against Russian pressure?	Should	43
	Should not	41
	Undecided	16
If the United States does decide to provide military supplies to Greece and Turkey, do you feel that this will make war with Russia more likely or less likely?	More likely	52
	Less likely	27
	No difference	10
	Undecided	11

As 1948 began, political crises in both Italy and France posed a possibility of Communist rule. While hopeful that Communist governments could be kept from coming to power, Americans between February 16 and 20 of that year again expressed no general enthusiasm for intervening militarily.

United States, February 16–20, 1948[13]

Do you think it is likely or unlikely that the Communists will take over the government of Italy (France) in the next year or two?

[12] "The Quarter's Polls," *Public Opinion Quarterly* 12 (Fall 1948): 542.

[13] Ibid., p. 537.

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	Italy	France
Likely	29.6%	17.8%
Unlikely	39.6	51.8
Don't know and no answer	30.8	30.4

Suppose the government of Italy seemed likely to be taken over by the Italian Communists in a revolution. Would you be willing to see United States military forces used to prevent this or not?

Yes	28.3%
No	52.2
Only through UN (volunteered)	5.5
Don't know and no answer	14.0

One then gets a substantially different response to similar questions in polls of June and July. Slight differences of wording may explain part of the change in response, but one also had a number of real-world events in the background, the Communist takeover of Czechoslovakia on February 25, 1948, and the blockade of Berlin in June.

United States, June 1948[14]

If it looked as though we could not keep France (Italy, Greece, Turkey) from becoming a Russian-dominated country in any other way, would you be in favor of sending a U.S. military force there or not?

	Yes	No	No opinion
France	56%	27%	17%
Italy	55	28	17
Greece	53	29	18
Turkey	52	29	19

1950

The Korean War then indeed put the capstone on cold war sentiment, by seemingly proving that Stalin's bloc would be willing to resort to more than a blockade, going on to outright military aggression. Berlin may have been at the center of everyone's attention in 1948 and 1949, while Korea was at the periphery, virtually overlooked in the policy statements of an Acheson or a MacArthur. Yet the precedent of military aggression then brought it into the center of attention.

Before 1950 it was hardly clear that the American people or government had seen a need yet for "containment" in Asia. But now the line of containment was to be drawn virtually everywhere. China in 1948 and 1949 drew no great endorsement for American moves to tilt the balance. It was indeed only after the outbreak of the Korean War that aid to Chiang Kai-shek's forces again drew majority approval:

[14] Ibid., p. 577.

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United States, January 1949[15]

Do you think the U.S. Government should or should not continue to help the Chiang Kai-shek (Nationalist) Government in the present struggle with opposing forces?	Should	29%
	Should not	54
	No opinion	17

United States, 1950–1951

Do you think the United States should or should not send war materials to Chiang Kai-shek and his Chinese Nationalist Government now in Formosa?

	Should	Should not	No opinion
August 1950	48%	35%	17%
January 1951	54	32	14

The Korean War played an important role similarly in erasing some lingering World War II images which were decidedly counter to the cold war. In 1949 one still saw persisting distrust of any German or Japanese reentry into active international politics:

United States, November 1949[16]

If there is another world war, do you think Japan will fight on the side of the United States or against the United States?

With us	Against us	Neutral	No opinion
38%	36%	1%	25%

If there is another world war, do you think the new Western Germany will fight on the side of the United States or against the United States?

With us	Against us	No opinion
43%	32%	25%

As in other cases, Korea thus served to dispel fears of "Axis rearmament" here, or at least to suggest that the gains of such rearmament, in heading off Moscow-directed invasions, were sufficient to outweigh the risks.

United States, September 1951[17]

Would you favor or oppose allowing the use of West German soldiers in the allied European army under General Eisenhower?

Favor	Oppose	No opinion
72%	15%	13%

Although Italy is a member of the North Atlantic organization, she is limited by peace treaty terms on the number of soldiers she can have. Would you favor or oppose allowing Italy to increase the size of her armed forces?

Favor	Oppose	No opinion
59%	25%	16%

[15] "The Quarter's Polls," *Public Opinion Quarterly* 15 (Summer 1951): 388.
[16] "The Quarter's Polls," *Public Opinion Quarterly* 14 (Spring 1950): 178-179.
[17] *Gallup Polls*, vol. II, p. 1017.

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Some Conclusions

No one can thus deny that there was a cold war, or that cold war assumptions captured the elite opinion and mass opinion of the United States. The outline just above is merely intended to suggest that such opinion took longer to settle into place than we sometimes assume in retrospect; quite possibly therefore it was a not-so-badly-taken response to the unfolding of real conflicts between the Soviet Union and the United States. As these conflicts did not reach their peak in 1946, but rather somewhere between 1948 and 1963, opinion peaked similarly.

Any reexamination of the cold war, as mentioned, verges often on becoming an exercise in epistemology. Who is free of "cold war biases" in considering this period? Who has been least brainwashed by his government, or by his ideological colleagues? At points, then, the sorting out of blame becomes a hopeless task, just as the plunge into the mutual destruction of the game-theoretical "prisoners' dilemma" situation defies an assessment of blame. Revisionists might thus succeed admirably in demonstrating doubts about Soviet blame, but should not fall into the trap of quickly replacing it with American blame.

If blame must be assessed, however, it at least behooves us to try to see events a little as contemporaries saw them, using this as a clue for trying to relieve our uncertainties. As suggested, the clues we can find here are more supportive for the "free world" position than is generally realized.

This has thus simultaneously been a plea for agnosticism about the blame for the cold war and a recognition that most observers will find it personally difficult to become totally agnostic. If, for good reasons or for bad, we will inevitably have opinions on this question, the debate between revisionist and establishment analyses is then bound to continue. Having been lured into this debate, this author a little reluctantly finds himself suggesting a reinforcement for the "establishment" analyses.

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The Effect of Incumbency on Voting in Congressional Elections, 1964–1974

CANDICE J. NELSON

The institutionalization of the House of Representatives early in the twentieth century marked the beginning of incumbency as an enduring characteristic of congressional elections. Between 1956 and 1976, 94 percent of all House members who sought reelection were reelected.[1] Moreover, during this same period the number of incumbents involved in close re-election contests sharply declined.[2] In 1976 only 16 percent of all House incumbents were re-elected with less than 60 percent of the vote, and only 9 percent were re-elected with less than 55 percent of the vote.[3]

As members of Congress have become increasingly invulnerable to electoral defeat, the advantage of incumbency in congressional elections has received greater scrutiny. Various studies have attempted to assess both the electoral advantage which incumbents possess and the causes of such advantage.[4] However, there has been very little examination of incumbency in relation

[1] Figures computed from data presented in *Congressional Quarterly Weekly Reports*, July 13, 1974, p. 1815, November 9, 1974, pp. 3065–69; November 6, 1976, pp. 3119–3122.

[2] David R. Mayhew, "Congressional Elections: The Case of the Vanishing Marginals," *Polity* 6 (Spring 1974): 304–305.

[3] Figures computed from data presented in *Congressional Quarterly Weekly Report*, November 6, 1976, pp. 3147–3154.

[4] Barbara Hinckley, "Incumbency and the Presidential Vote in Senate Elections: Defining Parameters of Subpresidential Voting," *American Political Science Review* 64 (September 1970); Warren Kostroski, "Party and Incumbency in Postwar Senate Elections: Trends, Patterns, and Models," *American Political Science Review* 67 (December 1973); Robert S. Erickson, "The Advantage of Incumbency in Congressional Elections," *Polity* 3 (Spring 1971); Andrew Cowart, "Electoral Choice in the American States: Incumbency Effects, Partisan Forces, and Divergent Partisan Majorities," *American Political Science Review* 67 (September 1973); John A. Ferejohn,

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to other factors known to affect congressional voting. Previous research has found that party identification, candidate familiarity, and presidential performance are important determinants of the outcome of congressional elections.[5] This paper uses survey data to explore the influence of incumbency on the relationship between these three variables and voting behavior in congressional elections between 1964 and 1974.

Incumbency and Defection

Early studies of the voting behavior of the American electorate found that party identification was the most important determinant of candidate choice in congressional elections. In their study of voting in the 1958 congressional election, Donald E. Stokes and Warren E. Miller found that only 5 percent of the Democrats and 6 percent of the Republicans deviated from their party affiliations in voting for congressional candidates.[6] However, during the past twenty years there has been an increase in the percentage of party identifiers who vote against their own party's congressional candidates. Robert B. Arseneau and Raymond E. Wolfinger found that defection in House elections increased from 9 percent in 1956 to 18 percent in 1974.[7] One explanation for this increase in defection may be that incumbency has become a more important voting cue, and party identification a correspondingly less important cue, in congressional elections.

If incumbency influences candidate choice in congressional elections, we would expect voters to be least likely to defect when their own party's candidate was an incumbent and most likely to defect when the other party's candidate was an incumbent.[8] Table 1 compares defection from party voting when voters and incumbents belong to the same party with defection when voters and incumbents belong to opposite parties.[9]

As we expected, the data presented in Table 1 show that, in every congressional election between 1964 and 1974, voters were least likely to defect

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"On the Decline of Competition in Congressional Elections," *American Political Science Review* 71 (March 1977); Albert D. Cover, "One Good Term Deserves Another: The Advantage of Incumbency in Congressional Elections" (Paper presented at the 1976 Annual Meeting of the American Political Science Association, Chicago, Illinois).

[5] Donald E. Stokes and Warren E. Miller, "Party Government and the Saliency of Congress," *Public Opinion Quarterly* 26 (Winter 1962): 531–546; Robert B. Arseneau and Raymond E. Wolfinger, "Turnout and Defection in House Elections" (Paper delivered at the 1973 Annual Meeting of the American Political Science Association); Edward R. Tufte, "Determinants of the Outcomes of Midterm Congressional Elections," *American Political Science Review* 69 (September 1975): 812–826.

[6] Stokes and Miller, "Party Government and the Saliency of Congress," p. 534.

[7] Arseneau and Wolfinger, "Turnout and Defection in House Elections," p. 13.

[8] Incumbents are defined as all candidates who were members of Congress at the time of the November election.

[9] All congressional districts in which only one major-party candidate sought election have been excluded from the analysis.

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TABLE 1

Vote Against Incumbent Candidates, by Party Identification, 1964–74 (percent)

	Incumbent of Own Party	*Incumbent of Opposite Party*	*Total*	*N*
1964	10	24	15	(146)
1966	7	36	18	(106)
1968	14	31	21	(160)
1970	6	30	16	(92)
1972	9	35	19	(209)
1974	11	32	20	(179)
Mean	10	31	18	

Note: Defection is defined as a vote for the congressional candidate of the other party by a party identifier. Pure Independents have been excluded, and Independent Democrats and Independent Republicans have been treated as party identifiers.

when their own party's candidate was an incumbent and were most likely to defect when the other party's candidate was an incumbent. If we compare the mean defection during the ten-year period we see that the mean percentage of voters who defected in the six congressional elections was 18 percent; yet the mean defection from one's own party's incumbent was only 10 percent, and the mean defection to the other party's incumbent was 31 percent.

If incumbency is to provide a partial explanation for the increase in defection from party voting in congressional elections, we would expect to observe a decrease in defections from one's own party's incumbent and an increase in defections to the other party's incumbent. While there does not seem to be a perceptible decrease in the percentage of voters who defected from their own party's incumbent during the ten-year period, there was a sharp increase in defections to the other party's incumbent between 1964 and 1966. Defection rose from 24 percent in 1964 to 36 percent in 1966, and remained at 30 percent or above from 1966 to 1974.

This increase in defections to the other party's incumbent between 1964 and 1966 corresponds to the increase in the incumbency advantage between 1964 and 1966 noted by Robert S. Erickson. Using aggregate data, Erickson found that the electoral advantage held by incumbents increased from 2 percent in 1964 to approximately 5 percent in 1966. The data presented in Table 1 lend further support to the increased power of incumbency from 1966 on.[10]

A third observation we can make about Table 1 is that anti-incumbent defection, that is, defection from one's own party's incumbent, was greater in the presidential-year elections of 1964 and 1968 than in the off-year

[10] Robert S. Erickson, "Malapportionment, Gerrymandering, and Party Fortunes in Congressional Elections," *American Political Science Review* 66 (December 1972): 1240. Albert Cover, using survey data, also found that there was a trend toward pro-incumbent defection from 1964 to 1974. Cover, "One Good Term Deserves Another," p. 15–16.

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elections of 1966 and 1970. In the 1966 and 1970 elections only 7 percent and 6 percent, respectively, of the voters defected from their party's incumbent congressional candidates, while in the presidential-year elections defection ranged from 9 percent in 1972 to 14 percent in 1968. Similarly, 36 percent of the voters supported the opposition party's incumbent candidates in 1966, compared with only 24 percent of the voters in 1964 and 31 percent of the voters in 1968.

This pattern of greater pro-incumbency voting in the 1966 and 1970 off-year congressional elections does not hold true, however, for the 1974 election. In 1974 defection from one's own party's incumbent was greater than such anti-incumbent defection in 1972, while defection to the other party's incumbent was less than such defection in 1972. In 1974 11 percent of the voters defected from their party's incumbent, compared with only 9 percent in 1972, whereas defection to the other party's incumbent dropped from 35 percent in 1972 to 32 percent in 1974.

The fact that pro-incumbency voting seemed to be greater in the 1966 and 1970 elections suggests that incumbency may be a more important force in the less salient, less visible off-year congressional elections. It is possible that incumbency, like party identification, serves as a long-term force in congressional elections, and the consequent importance of incumbency as a voting cue varies in relation to the short-term forces present in any election year. Therefore, we would expect incumbency to be a less important determinant of congressional vote in presidential-year elections, because of the stimulus of the presidential campaign, the appeal of the presidential candidates, and the greater salience of issues.

This conceptualization of incumbency as a long-term force in congressional elections would also explain the deviation observed in the 1974 election from the pattern of pro-incumbency voting noted in the 1966 and 1970 off-year elections. If we view the events surrounding the Watergate affair and the problems of the Nixon administration as short-term forces in the 1974 congressional election, then it is not surprising that the pattern of incumbency voting in the 1974 election was more like the pattern noted in the earlier presidential-year elections.[11]

Previous studies of voting behavior in congressional elections have found that defection from party voting increases as strength of party identification decreases. If incumbency serves as an alternative voting cue to party identification in congressional elections, we would expect the importance of incumbency to increase as strength of partisanship decreases. Table 2 presents the relationship between incumbency, strength of partisanship, and defection.

When we examine the data presented in Table 2 we notice first that regardless of the party affiliation of the incumbent candidates strong partisans

[11] For a discussion of the effect of the abnormal circumstance surrounding the 1974 congressional election on incumbent candidates, see Walter Dean Burnham, "Insulation and Responsiveness in Congressional Elections," *Political Science Quarterly* 90 (Fall 1975): 411.

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TABLE 2

Vote Against Incumbent Candidates, by Strength of Party Identification, 1964–74 (percent)

	Incumbent of Own Party			
	Strong Partisans	*Weak Partisans*	*Independent Partisans*	*N*
1964	3	18	19	(484)
1966	3	8	17	(273)
1968	9	17	20	(381)
1970	2	9	9	(298)
1972	4	11	13	(577)
1974	10	12	13	(395)
Mean	5	13	15	
	Incumbent of Other Party			
	Strong Partisans	*Weak Partisans*	*Independent Partisans*	*N*
1964	14	30	29	(337)
1966	22	40	49	(197)
1968	14	40	42	(314)
1970	21	32	44	(228)
1972	22	40	44	(354)
1974	19	41	32	(341)
Mean	19	37	40	

are less likely to defect from their party's congressional candidates than are weak and independent partisans. Between 1964 and 1974 an average of 5 percent of the strong partisans defected from their party's incumbent congressional candidates, while 13 percent of the weak partisans and 15 percent of the independent partisans defected. Similarly, the mean defection to the other party's incumbent was 19 percent for strong partisans, compared with 37 percent for weak partisans and 40 percent for independent partisans.

While the likelihood of defection from party voting increases as strength of partisanship decreases, incumbency also affects the likelihood of defection. All three categories of partisans were more likely to defect when the other party's candidate was an incumbent than when their own party's candidate was an incumbent. Moreover, the importance of incumbency increased as strength of partisanship decreased. If we calculate the difference between the percentage of strong, weak, and independent partisans who defected from their own party's incumbent and the percentage who defected to the other party's incumbent, we find that the mean difference in defection was 14 percent for strong partisans, 24 percent for weak partisans, and 25 percent for independent partisans.

The findings presented in Table 2 lend strong support to the supposition that incumbency is more important to voters who lack strong party affiliations. In light of these findings, we would expect incumbency to be most important to voters who have no party affiliation. Table 3 presents the percentage of Independents who voted for incumbents between 1964 and 1974.

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TABLE 3

Independents' Vote for Incumbents, 1964–1974

	Percent Voting for Incumbents in Democratic Districts	*Percent Voting for Incumbents in Republican Districts*	*N*
1964	81%	39%	(46)
1966	59	62	(45)
1968	59	75	(60)
1970	71	78	(50)
1972	70	70	(106)
1974	72	69	(85)
Mean	69	66	

Note: Independents are defined as voters with no partisan affiliation or leaning.

As we expected, over half of all Independent voters voted for incumbent candidates. In districts with Democratic incumbents, an average of 69 percent of the Independents voted for the incumbent; in districts with Republican incumbents, an average 66 percent of the Independents voted for the incumbent. Moreover, the tendency of Independents to vote for an incumbent, regardless of the party affiliation of the incumbent, is found in every year except 1964, when only 39 percent of the Independents voted for Republican incumbents. However, the disinclination of Independents to vote for Republican incumbents in 1964 most likely reflects the overwhelming general rejection of Republican candidates in the 1964 elections.

The data presented thus far have shown that incumbency restrains the tendency of party identifiers to cast their congressional votes along party lines. Moreover, we have found that the importance of incumbency increases when other, more salient voting cues, such as party identification, are weakened or absent. However, it may be that incumbency results in these consequences only when voters know who the incumbent candidates are. To ascertain whether the effect of incumbency depends upon a voter's awareness of the candidates, or occurs independently of voter awareness, we need to examine the relationship between incumbency, awareness of congressional candidates, and defection.

Candidate Salience, Incumbency, and Defection

We have seen that incumbent candidates have an electoral advantage over their nonincumbent opponents. One possible explanation for this advantage may simply be that voters tend to support candidates they know, and incumbents are better known than nonincumbents. David Mayhew has suggested that incumbent candidates, through increased use of the franking privilege and more sophisticated polling techniques, are becoming more adept at making

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TABLE 4

Awareness of Incumbent and Nonincumbent Candidates, 1964–74 (percent)

	Know Incumbent	*Know Nonincumbent*
1964	51 (643)	39 (73)
1966	37 (351)	43 (85)
1968	40 (531)	36 (55)
1970	33 (411)	32 (40)
1972	35 (271)	28 (52)
1974	34 (610)	25 (83)
Mean	38	34

Note: Figures in parentheses show the number of respondents who know the incumbent or the nonincumbent.

themselves known to their constituents.[12] Table 4 shows, as we expected, that incumbent candidates are better known than are nonincumbents.[13]

However, the data presented in Table 4 also show that incumbent candidates were no more well known to their constituents in 1974 than they were in 1964. In fact, the electorate's knowledge of incumbent candidates declined from 51 percent in 1964 to 34 percent in 1974. It seems that despite increased use of the perquisites of their office, House members were unable to translate these resources into greater voter recognition.

Arseneau and Wolfinger found that individuals are more likely to know their own party's candidate than the other party's candidate.[14] The data presented in Table 5 show that although individuals are more likely to know their own party's nonincumbent candidate than the other party's nonincumbent, in every year except 1972 individuals were just as likely to know the other party's incumbent as to know their own party's incumbent. This finding suggests that incumbents are able to use the advantages of their office to make themselves known to their constituency, regardless of the partisan leanings of the constituents. While partisans are more likely to know their own party's candidates than the other party's candidates, incumbent candidates are able to overcome the difference in awareness and make themselves equally well known both to constituents with whom they share and to constituents with whom they do not share party affiliation. Incumbency, in effect, negates the effect of party identification on a voter's awareness of congressional candidates.

The first section of this paper presented evidence that incumbency did affect voting in congressional elections. The data presented in Table 5 have

[12] Mayhew, "Congressional Elections," p. 311.

[13] An individual was considered to know a congressional candidate if he could give the correct name of the candidate when asked to do so by an interviewer.

[14] Arseneau and Wolfinger, "Turnout and Defection in House Elections," p. 6.

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TABLE 5

Awareness of Incumbent and Nonincumbent Candidates, by Party, 1964–74 (percent)

	Know Incumbent		*Know Nonincumbent*	
	of Own Party	*of Opposite Party*	*of Own Party*	*of Opposite Party*
1964	53 (691)	53 (472)	42 (123)	34 (50)
1966	39 (480)	40 (322)	54 (79)	37 (100)
1968	40 (659)	43 (516)	43 (75)	29 (56)
1970	33 (612)	36 (471)	40 (57)	22 (51)
1972	40 (423)	29 (269)	37 (94)	19 (54)
1974	37 (829)	36 (636)	27 (149)	26 (118)
Mean	40	40	41	28

Note: Figures in parentheses indicate the total number of cases in each category on which the percentages are based.

shown that an individual is equally likely to know the incumbent candidates of his own and of the opposition parties, but is more likely to know a nonincumbent candidate if the nonincumbent and the individual share party affiliation. The question which remains to be answered, however, is whether the effect of incumbency on congressional vote is dependent upon a voter's knowledge of the incumbency status of the candidate, or whether it influences congressional vote independently of a voter's awareness of who the incumbent candidate is. The data presented in Table 6 examine the relationship between incumbency, candidate salience, and defection.

When we compare defection from party voting among voters who knew both candidates, we notice, as we would expect, that defection was least when a voter's own party's candidate was an incumbent, and was greatest when the other party's candidate was an incumbent. Between 1964 and 1974 an average of only 16 percent of those voters who knew both congressional candidates defected from their own party's incumbent, while defection among those voters averaged 30 percent when the incumbent was affiliated with the other party. This pattern also held true among voters who knew neither party's candidate. Despite the fact that voters in this last category could not recall the names of either candidate, they still were least likely to defect from their own party's candidate when that candidate was an incumbent and were most likely to defect from their own party's candidate when the other party's candidate was an incumbent. Mean defection among voters who knew neither candidate was only 10 percent when the incumbent belonged to their own party, yet was 23 percent when the incumbent identified with the other party. These findings indicate that incumbency does affect congressional voting independent of a voter's ability to recall the name of the incumbent.[15]

[15] John Ferejohn also found that in the 1964–1970 congressional elections incumbency had an

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TABLE 6

Vote against Incumbent Candidates, by Party and by Awareness of Candidates' Names, 1964–74 (percent)

	Knows Candidate of Own Party	*Knows Candidate of Other Party*	*Knows Both Candidates*	*Knows Neither Candidate*
1964				
Incumbent of Own Party	5 (149)	17 (6)	14 (162)	10 (157)
Incumbent of Other Party	22 (9)	43 (51)	25 (155)	16 (114)
1966				
Incumbent of Own Party	4 (84)	a	7 (69)	8 (118)
Incumbent of Other Party	0 (6)	62 (37)	38 (71)	24 (83)
1968				
Incumbent of Own Party	3 (89)	20 (5)	21 (141)	14 (136)
Incumbent of Other Party	9 (11)	56 (52)	26 (134)	27 (117)
1970				
Incumbent of Own Party	0 (102)	a	17 (52)	6 (140)
Incumbent of Other Party	7 (14)	55 (51)	26 (76)	21 (86)
1972				
Incumbent of Own Party	5 (102)	a	12 (57)	10 (417)
Incumbent of Other Party	17 (6)	75 (24)	33 (39)	32 (285)
1974				
Incumbent of Own Party	0 (132)	40 (5)	25 (97)	12 (161)
Incumbent of Other Party	22 (18)	59 (65)	33 (117)	19 (141)
Mean				
Incumbent of Own Party	3	26	16	10
Incumbent of Other Party	13	58	30	23

Note: Figures in parentheses show the number of cases on which the percentages are based.
[a]Less than 5 cases in cell.

Studies of voting behavior in congressional elections have found that voters are least likely to defect from their party's candidate when they do not know the other party's candidate and are most likely to defect from their party's candidate when they know only the opposition party's candidate.[16] If the

effect on voting independent of a voter's awareness of the congressional candidates. Ferejohn concluded that "voters were apparently using incumbency as a voting cue whether or not they could recall the names of the incumbent candidate in the interview situation." Ferejohn, "On the Decline of Competition in Congressional Elections," p. 171.

[16] Arseneau and Wolfinger, "Turnout and Defection in House Elections," p. 17.

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effect of incumbency on congressional vote were dependent upon a voter's knowledge of who the incumbent candidate is, we would expect that voters who knew only their own party's candidate would not defect regardless of the party affiliation of the incumbent. Yet Table 6 shows that in every year except 1966 voters who knew only their own party's candidate were nevertheless more likely to defect when the incumbent belonged to the other party than when the incumbent identified with the voter's party. Mean defection among voters who knew only their own party's candidate was only 3 percent when their own party's candidate was an incumbent, yet was 13 percent when the other party's candidate was an incumbent. Similarly, the mean defection among voters who knew only the other party's candidate was 26 percent when their own party's candidate was the incumbent, but rose to 58 percent when the other party's candidate was the incumbent. However, while these data further suggest that incumbency affects congressional vote even when awareness of the incumbent candidate is absent, the cell sizes are too small to draw more than speculative conclusions.

One possible explanation for the fact that incumbency influences defection from party voting in congressional elections even among voters who do not know the name of the incumbent could be that state ballots designate the incumbency status of congressional candidates. If the incumbent congressional candidates were indicated on a voter's ballot, for example, by such designations as "incumbent," "candidate for re-election," or even "Member of Congress," the apparent independent effect of incumbency on congressional vote would be explained. Though a voter might be unable to remember the name of the incumbent candidate during an interview, he would have that knowledge at his disposal in the voting booth, and thus would, in fact, know who the incumbent candidate was when he cast his vote.

However, this explanation does not account for the apparent independent effect of incumbency on vote which we observe in Table 6, because only two states, California and Massachusetts, indicated the incumbency status of congressional candidates on the ballot between 1964 and 1974.[17] When Table 6 was recomputed, excluding the voters from California and Massachusetts, the findings remained the same. These results suggest that there is a name recognition factor which accompanies incumbency, enabling voters to recognize the incumbent candidate when they see his or her name on the ballot, even though they cannot recall the name of the incumbent in an interview.

Incumbency, Evaluation of the President, and Defection

Several recent studies of voting in congressional elections have found that an individual's evaluation of how well an incumbent president is performing his

[17] Information concerning ballot format was obtained from the election codes for each of the fifty states. Massachusetts designated incumbents *candidates for re-election;* California used the designation *incumbent.*

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TABLE 7

Vote against Incumbent Candidates, by Party and by Voters' Evaluation of the President, 1968–74 (percent)

	President and Voter of Same Party						*President and Voter of Opposite Parties*					
	Incumbent of Own Party			*Incumbent of Other Party*			*Incumbent of Own Party*			*Incumbent of Other Party*		
	Good[a]	*Fair*	*Poor*	*Good*	*Fair*	*Poor*	*Good*	*Fair*	*Poor*	*Good*	*Fair*	*Poor*
1968	15	21	28	31	29	67	7	12	4	38	27	18
	(132)	(58)	(22)	(110)	(55)	(15)	(30)	(82)	(50)	(29)	(55)	(40)
1970	5	4	b	20	33	b	18	4	4	48	34	32
	(104)	(23)		(76)	(12)		(33)	(78)	(54)	(25)	(61)	(50)
1972	10	19	33	28	57	b	13	8	2	57	30	23
	(185)	(32)	(6)	(160)	(21)		(102)	(119)	(94)	(54)	(53)	(35)
1974	16	22	b	43	20	b	9	7	3	38	31	0
	(115)	(50)		(100)	(41)		(88)	(92)	(40)	(63)	(94)	(33)
Mean	12	17	b	31	35	b	12	8	3	45	31	18

Notes: Respondent's evaluations of the president were not available for 1964 and 1968. Figures in parentheses show the total number of cases in each category, not merely the number defecting.

[a] Evaluation of the president.

[b] Less than 5 cases in cell.

job has an important effect on his vote for Congress. Edward Tufte used aggregate data to examine voting in midterm congressional elections between 1938 and 1970 and found that presidential popularity was one of two factors which explained virtually all of the variance in voting in congressional elections.[18] Arseneau and Wolfinger used survey data to show that in the 1968, 1972, and 1974 congressional elections dissatisfaction with the president's performance in office was paralleled by defection from his party's congressional candidates.[19] However, in light of the findings presented in this paper, we might expect the presence of an incumbent congressional candidate to moderate the relationship between evaluation of the president and congressional vote.

Table 7 shows the effect of incumbency on the tendency of voters who are dissatisfied with a president's performance to vote against his party's congressional candidates. We notice, first, that even when we take into account the incumbency status of congressional candidates, defection in congressional elections is related to the voter's evaluation of the president. When the president and the voter belong to the same political party, defection from party voting increases as positive evaluations of the president decline. Between 1968 and 1974, as evaluations of the president moved from good to fair, the mean defection from a voter's own party's incumbent increased

[18] Tufte, "Determinants of the Outcome of Midterm Congressional Elections," p. 817.

[19] Arseneau and Wolfinger, "Turnout and Defection in House Elections," pp. 21–22.

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from 12 percent to 17 percent, and the mean defection to the other party's incumbent rose from 31 to 35 percent. Similarly, when the president and the voter have different party affiliations, defection decreases as negative evaluations of the president increase. During the same six-year period, as evaluations of the president changed from good to poor, the mean defection from a voter's own party's incumbent declined from 12 percent to 3 percent, and defection to the other party's incumbent decreased from 45 percent to 18 percent.

Despite the fact that voting in congressional elections is a reflection of attitudes toward presidential performance, incumbency nevertheless seems to moderate the relationship between evaluation of the president and congressional vote. For example, the data presented in Table 7 show that when the president and the voters belonged to the same political party, voters who were dissatisfied with the president were still less likely to defect when their party's congressional candidate was an incumbent than when the other party's candidate was an incumbent. Among voters who thought the president was doing only a fair job the mean defection was 17 percent when incumbents and voters shared party affiliation, but rose to 35 percent when the other party's congressional candidate was the incumbent.

If we examine defection from party voting when the president and the voters belonged to opposite parties, we notice once again that incumbency modifies the effect of evaluation of the president on congressional vote. Voters who approved of the president's performance were still less likely to defect when the incumbent candidate belonged to their party than when the incumbent candidate belonged to the opposite party. Between 1968 and 1974 an average of only 12 percent of those voters who thought the president was doing a good job defected from their own party's incumbent, compared with 45 percent who defected when the other party's candidate was an incumbent. Similarly, the mean defection among voters with poor evaluations of the president was 3 percent when a voter's own party's candidate was the incumbent, yet was 18 percent when the other party's candidate was an incumbent.

The data presented in Table 7 tend to support the thesis that voting in congressional elections is actually a referendum on presidential, rather than congressional, performance. Yet, while congressional candidates in general seem to reap the consequences of a president's actions, incumbency tempers the relationship between evaluations of the president and congressional voting.

Conclusions and Implications

The findings presented in this paper confirm the importance of incumbency as a factor in congressional elections. We have found that incumbent candidates have an electoral advantage over their nonincumbent opponents. The data showed that incumbency restrains the tendency of party identifiers to

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cast their congressional votes along party lines, and increases in importance as strength of partisanship declines.

We speculated earlier in this paper that incumbency is a long-term force in congressional elections. If this is true, we would expect the importance of incumbency to vary in relation to the other long-term and short-term forces present in each electoral contest. The data presented in this paper suggest that the relative importance of incumbency is affected by two factors: short-term forces which are salient during the election period, and the partisanship of the voters.

Our findings suggest that incumbency becomes an important voting cue when short-term forces, such as salient issues or the activity of a presidential campaign, are lessened or absent. We found that incumbency was most important in the 1966 and 1970 congressional elections, elections which lacked both the stimuli of the presidential campaigns of 1964, 1968, and 1972, and the controversy surrounding the Watergate affair which preceded the 1974 elections. If the importance of incumbency does vary in relation to the short-term forces present in a congressional election, we would expect incumbency to be a more important factor in less salient, less visible off-year congressional elections than in presidential-year elections. While our findings lend some support to this hypothesis, study of a broader time period is necessary before any firm conclusions can be drawn.

While short-term forces seem to affect the importance of incumbency in congressional elections, the relative importance of incumbency is also affected by the partisanship of the electorate. As a voting cue, incumbency serves as an alternative to party identification. Voters who lack strong partisan affiliations may rely on incumbency, rather than party identification, to guide their votes in congressional elections. Interestingly, the increased electoral advantage of incumbents between 1964 and 1966, reported in Erickson's work, occurred at the same time that the number of Independents, Independent Democrats, and Independent Republicans in the population began to increase. The findings presented in this paper suggest that the increased electoral advantage of incumbents from 1966 on is at least in part a consequence of the increase in the number of partisan Independents and Independents in the population.

One consequence of the changing partisanship of the population may be that incumbency is becoming more important, and party identification correspondingly less important, in congressional elections. We can find further support for this supposition in the perceptual advantage incumbent candidates have over their nonincumbent opponents. The data showed that not only were incumbent candidates better known than nonincumbents, but also that individuals were just as likely to know the other party's incumbents as to know their own party's incumbents. While past studies have found that individuals are more likely to know their own party's congressional candidates than the opposition party's candidates, our findings indicate that when incum-

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bency is taken into account, incumbency, rather than party identification, becomes the more important identity link between an incumbent and his constituents.

Finally, when we examined the effect of incumbency on a voter's tendency to use his congressional vote as an expression of approval or disapproval of the incumbent president's performance in office, we found that incumbency restrained the tendency of congressional voting to be a referendum on the president's performance. We found that while defection from the congressional candidates of the president's party increased as negative evaluations of the president increased, defection nevertheless was reduced when incumbents belonged to the president's party and was increased when incumbents belonged to the opposite party. It seems that although incumbent candidates in large measure are held accountable for the actions of the president, rather than the Congress, their incumbency provides a countervailing force which mediates the consequences of public attitudes toward the president's performance.*

* This is a revised version of a paper presented at the 1976 Annual Meeting of the American Political Science Association, Chicago, Illinois. I thank Raymond E. Wolfinger, David J. Vogler, and Steven J. Rosenstone for their comments on an earlier draft of this paper.

The data used in this paper were collected by the University of Michigan Center for Political Studies and made available through the Inter-University Consortium for Political Research. The author is solely responsible for the analysis and interpretation of the data.

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Book Reviews

The Liberal Heresy: Origins and Historical Development *by Massimo Salvadori. New York, St. Martin's Press, 1978. 248 pp. $18.95.*

As the title of Massimo Salvadori's new book indicates, liberalism has been a controversial, and never fully accepted, political point of view, even in liberal countries themselves. In part, this is because liberalism, at its very core, strongly supports dissident opinions and minority claims. Justifying liberalism's heretical spirit is one of the main tasks of Salvadori's study whose principal aim is to defend liberalism against its critics on the left and on the right. Liberalism today is surely in need of such a defense; over the years, it has been weakened greatly by attacks of both radicals and conservatives who defend, from their different perspectives, the need for authoritarian government.

The Liberal Heresy seeks to illuminate the central meaning of liberalism by tracing its development and by showing the connection between liberal values and the need for liberal institutions. Salvadori sees liberalism primarily as a philosophy of emancipation, freeing the individual from the constraints of environment and culture. As liberalism struck roots, especially in the English-speaking world, it unleashed dormant energies, providing the foundation for an altogether new kind of civilization; one that stressed, for the first time, growth, creativity, and change. Prior to liberalism, custom and hierarchy held exclusive sway. Thus, according to Salvadori, liberty was the supreme principle underlying the liberal point of view; all other values were secondary. But liberty must not remain a vague abstraction or hazy ideal; specific institutions are necessary to make it concrete. Two stand out: freedom of expression, and political freedom—the right to assemble, vote, hold office, and be governed through a constitution whose powers were divided and whose implementation was dependent on the opinions of the citizens themselves.

Although Salvadori has selected a topic that today deserves much more attention, his account of liberalism suffers for two reasons. Salvadori's discussion of liberalism is largely a caricature: liberalism represents everything worthwhile—emancipation, tolerance, reason, creativity, legality, fraternity, duty; opponents of liberalism represent

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everything bad. Although Salvadori announces at the beginning of the book that he will examine the strengths and weaknesses of liberalism in a later volume, we have a right to expect the author to show a real sensitivity to the genuine problems liberalism gives rise to, especially when the chief aim of *The Liberal Heresy* is to defend liberalism against its detractors. Yet Salvadori shows no such awareness.

The other great drawback of *The Liberal Heresy* is that Salvadori portrays an awkwardness or inability to handle ideas—to uncover their underlying logic and root them in experience. Instead, he merely catalogues the main ideas of liberalism. When Salvadori occasionally does try to unravel the basic reasons why an idea is important, his explanations are inadequate; sometimes, they are, in fact, circular. For example, in attempting to explain the meaning of equality, Salvadori writes that "many liberals have found a satisfactory explanation for equality in Kant's postulate (expressed in the categorical imperative) that those moral principles are valid which . . . embody the assumption that human beings are equal" (p. 29).

The Liberal Heresy addresses a genuine need: a clear explanation of what liberalism is; why it has fallen on hard times; and why, nevertheless, its preservation remains important for clarifying our present political situation and reflecting on the goals we should strive to attain for the future. Making sense of liberalism becomes ever more urgent in an age in which a dogmatic commitment to behavioralism, pragmatism, or various species of utopianism eschews political philosophy. But the job of explaining liberalism and making it convincing to its critics, or to those who straddle the fence, is a task still to be completed.

PETER STERN
New School for Social Research

Chance and Circumstance: The Draft, The War, and The Vietnam Generation

by Lawrence M. Baskir and William A. Strauss. New York, Alfred A. Knopf, 1978. 312 pp. $10.00.

This study chronicles how the Vietnam generation—those men who were of draft-eligible age between August 4, 1964, the date of the Tonkin Gulf Resolution, and March 28, 1973, when the last American troops departed Vietnam—encountered the government's military manpower procurement policy. Lawrence Baskir and William Strauss, former staff members of President Ford's Clemency Board, offer a clear statistical portrait of the fate of these men that will go far to rectify commonly held misunderstandings and stereotypes.

Of the approximately 27 million draft-age men of the Vietnam generation, only 11 million served in the military. Through a series of flow charts, Baskir and Strauss trace the statistical pattern of those who received selective service deferments, exemptions, or disqualifications; those who violated draft regulations; and those who entered the military and were subsequently accused of military offenses. The picture that emerges is

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a sobering one, belying any claims that the manpower to wage the war was raised in a just and equitable manner.

Baskir and Strauss depict a beleaguered selective service system caught in a vise between intensifying draft avoidance and resistance and an increasingly rigorous judiciary that placed procedural checks upon the draft. The growing sophistication of draft counseling, the developing expertise of attorneys in analyzing selective service regulations, and the wholesale errors committed by local draft boards in applying the law combined to make prosecution of draft cases a virtual no win proposition. Of the 210,000 accused offenders, most had their cases dropped; of the 8,750 convicted, 5,500 received either suspended sentences or probation, with 3,250 being imprisoned. By 1971, the enforcement of the draft act "was in a shambles" (p. 68). This judicial breakdown did not prevent the selective service system from delivering men to the armed forces. What it did insure was that those men who were not willing to serve and who had access to good counseling and legal representation were virtually assured of not being called. The burden of service, therefore, fell increasingly upon those who were poorer and less well educated.

Baskir and Strauss provide an enlightening treatment of those men who entered the military and were eventually accused of offenses. The deserters, who evoked such strong feelings during the amnesty debate, were the "military counterpart" (p. 109) of the draft resisters. Combat-related desertions were proportionally fewer than in previous wars. Only 5,000 soldiers were discharged for deserting during a Vietnam tour, about half of whom were men who failed to return from a rest and recuperation leave. There were only 24 proven cases of battlefield desertion. There were, however, 7,000 cases of desertion after orders were received for assignment to Vietnam, as well as 20,000 desertions of men returned from the war. By 1972 the desertion rate was 70 per thousand compared to 25 per thousand during the Korean War. The military, as well as the selective service system, was clearly in crisis; 250,000 men, only 13 percent of whom were convicted by court-martial, were punished with undesirable, bad conduct, or dishonorable discharges. Another 300,000 received general discharges, which are technically honorable but can prove a liability in seeking employment.

Baskir and Strauss present a detailed analysis of the programs of the Ford and Carter administrations that sought a redress of the grievances of those accused of draft or military violations. Both plans are criticized in regard to goals and implementation. Presidents Ford and Carter are each accused of not making a full commitment to "bring about a true national reconciliation" (p. 235).

The study is marred by a few errors that do not detract from the overall effect of its conclusions. The authors describe the New York City Civil War draft riots as having caused 1200 deaths (p. 18). This figure has often been cited, but Adrian Cook's definitive account, *The Armies of the Streets*, establishes that the highest possible death toll in that uprising was 119. Baskir and Strauss also list 1948 as the date of the nation's first peacetime draft act (p. 20) rather than the Selective Training and Service Act of 1940. Finally, General Lewis B. Hershey became the director of the selective service system in 1941, not 1947 (p. 15).

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Chance and Circumstance furnishes the precision and detail that has often been lacking in studies of the Vietnam generation. It provides a statistical framework that will prove increasingly beneficial as scholars seek some larger understanding of the Vietnam War and its impact upon American life.

JOHN O'SULLIVAN
Florida Atlantic University

Dragon and Eagle—United States-China Relations: Past and Future *edited by Michel Oksenberg and Robert B. Oxnam. New York, Basic Books, 1978. 384 pp. $13.50.*

This book consists of a series of papers written as a part of a project on "Sino-American Relations in Historical Perspective" sponsored by the China Council of the Asia Society. The thirteen contributors represent a considerable range of different scholarly interests, and they include a number of the more prominent names in recent sinology in this country.

The book is predominantly historical in character. Most of the authors devote the greater part of their analyses to reviewing past events, particularly of the past quarter century. The informed scholar will probably find little that is new in these historical analyses; but he will find the current conventional wisdom presented in a form that is usually clear and persuasive. The book has, however, more than simply a historical concern. The history is recounted in order to illuminate the policy questions which, one senses, are the real preoccupation of most of the authors. The editors in their introductory essay argue that the greatest need in current U.S. thinking about China is for "a new way of thinking" (p. 45); and the implication is that policymakers and the public alike will come to see the policy questions differently if they acquire the proper historical perspective.

The policy recommendations vary somewhat from one author to the next, but the variation is not large. In the main, the contributors agree about what ought to be the orientation of U.S. China policy. They agree—and this is a central message of the book—that policy needs to be made from the perspective of long-term objectives and expectations. Historically, policy has been made both in the United States and in China too much in terms of immediate needs and problems. The specific objectives which should govern U.S. policy, in turn, include: inclusion of the People's Republic as an equal in the structure of international relations; encouragement of its internal stability and prosperity; effective liaison between the Chinese and American *people;* and active pursuit of common interests throughout the world. The principal obstacle to the pursuit of these objectives has been, of course, the Taiwan problem, and there is repeated criticism throughout this book of American unwillingness to break its ties with Taipei. None of the authors feels that a complete break with Taipei is either necessary or desirable, but what is proposed is to do what is required to meet Peking's conditions for full normalization of relations. Robert Oxnam summarizes the general mood concisely when he says, "U.S. recognition of the Peking government is long overdue" (p. 29).

The arguments presented in support of these recommendations are familiar; for example, the more the People's Republic becomes involved in the framework of international

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relations, the more it can be counted on to stabilize the international system and to contribute to the solution of problems—and they have definite force. Precisely because of the perspective from which this book is written, however, the case made is not necessarily the strongest one that could be made for normalization. The danger to which the area specialist is likely to be vulnerable as a policy adviser is exaggeration of the significance of the nation or nations about which he has expertise; and there has been considerable evidence of this in the recent discussion of U.S. foreign policy by sinologists. One cannot make a truly effective case for a significant change in China policy simply by taking for granted its importance. If there is an argument to be made today for "benign neglect" in relation to China, its strongest defense is probably that China is not now a major power, and will not be for the foreseeable future. The only way to meet this, in turn, is by developing a broad geopolitical analysis that does not have China as its central focus. If there is any criticism to be made of this otherwise able book, therefore, it is that it presents simply a sinologist's perspective. What the sinologists have to say should certainly play a major role in shaping U.S. China policy, but it will always be one consideration among others.

BRUCE DOUGLASS
Georgetown University

Israel: The Embattled Ally *by Nadav Safran. Cambridge, Mass. The Belknap Press of Harvard University Press, 1978. 633 pp. $18.50.*

There has long been a need for a comprehensive study of U.S.-Israeli relations. While this book has elements which commend it, it is disappointing overall. It will be primarily of value to the non-Middle Eastern generalist and to students in courses on Israel in the Middle East. And even for them there are difficulties. To begin with, this is really three or even four books in one: an overview of aspects of Israeli society; a diplomatic history of Israel; a military history of the four Arab-Israeli wars; and a history of the U.S.-Israeli relationship with perhaps one-fourth of the book devoted to this topic. Trying to cover all four topics even within 600 pages is to attempt too much. The result is that the book is often either too general (as in the discussion of domestic Israeli politics) or too detailed (as in the diplomatic history of the years from 1967 to 1975 or the various wars from 1948 to 1973). At times the nonspecialist may have difficulty following some of the discussion as when names are introduced without being identified (p. 187) or when the elements which made up the Ma'arakh are listed two ways (pp. 169 and 176). More seriously, chapters do not build on each other logically and little or no attempt is made in the course of the book to weave the discussion into some overall theme. Nonetheless, Safran knows a great deal about Israel and there are often some very interesting and even thought-provoking sections. One example would be Chapter 18 which deals with the influence of security concerns and the military on Israeli society. The nonspecialist will learn much from reading the book, even with its problems.

With the possible exception of the discussion of the diplomatic history before and after the 1973 war which covers some new ground, the specialist, on the other hand, will find

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the book of much less interest. This is for two reasons: First, much of the rest of the book, most of it in fact, has been covered by Safran in his previous writings, especially in *From War to War* and *The United States and Israel*. Thus, "Book One: the Evolution of Israel" which ends on page 330 and the first 80 pages of "Book Two: Israel and America in International Relations" contain little which is new either in substance or interpretation. Second, amazingly, the book contains no footnotes or other citations. While there is a comprehensive bibliography of books (no articles) appended, there is virtually no reference made to them in the body of the text. Replication is at the heart of the scholarly enterprise. The absence of any citation makes evaluation and testing of many of Safran's assertions virtually impossible. The failure to cite sources leads to phrases which I found downright annoying. For example, he mentions records of the Eisenhower years "that have come to light" without indicating what they are (p. 371). He also refers to the contents of a note from Mrs. Meir to President Nixon saying euphemistically that "its substance can be fairly assessed from circumstantial evidence" (p. 482). What evidence? How circumstantial? Aside from the problem of replication, this approach avoids or ignores competing interpretations presented by other scholars. Two examples: The nature of Kissinger's role in the negotiations following the 1973 war and the contrasting U.S.-Israeli views on the shape of a final settlement have been the subject of much debate. Safran's interpretations are interesting but he does not explicitly make reference to those whose views differ from his. (One thinks of Edward Sheehan in the first case or George Ball in the second.) This is a pity since to have done so might have resulted in a deeper analysis than we get from a chronological recitation of the diplomatic interaction.

While there is simply not space in such a short review to do justice to the variety of topics Safran covers, I should like to touch on what seems to be his basic interpretation of the U.S.-Israeli relationship. Safran argues that after their relationship passed through many stages the United States and Israel were in "virtual alliance" with the signing of Sinai II (p. 576) since the United States made a series of commitments to Israel at that time and ended up being the guarantor of the agreement. Safran then ends his book with the assertion that any comprehensive peace "on the Israeli side will probably require nothing less than . . . a formal American-Israeli mutual security pact on the model of the American-Japanese Treaty" (p. 598). I have questions about this interpretation. It seems to me that Sinai II may have marked the end of whatever alliance there once may have been (if alliance there ever was). The United States, after all, became guarantor for both Egypt and Israel. Since then, she has come to challenge ever more explicitly the Israeli conception of peace while assiduously pursuing closer ties with many of Israel's adversaries. Just as the ties developed incrementally, one senses the relationship is disintegrating by degrees. In any case, it does not seem to me that Safran has made his case that an alliance does exist and certainly the future feasibility of one seems more and more problematical.

LEWIS BROWNSTEIN
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The Marshall Plan Summer: An Eyewitness Report on Europe and the Russians in 1947 *by Thomas A. Bailey. Stanford, Hoover Institution Press, 1977. 246 pp. $10.95.*

Thomas A. Bailey went to Europe in the summer of 1947, and the notes he made at the time are the basis of this book. Bailey tells us that the trip was offered to him as an inducement to teach at the National War College. So far as I can determine, Bailey had no assignment or charge, no duties to perform, and no obligation to make a report after his return. Neither is it evident that he lectured at the War College about his trip. In any case, Bailey refers to his notes in such a way as to suggest that he neglected them until he decided to deposit them in the Hoover Institution at Stanford, only to conclude that few people would know they were there and that his "pencilled jottings" were "so cryptic, cramped, and smudged" that only he could decipher them (p. vii).

Between June 11 and August 18, 1947, Bailey visited Ireland, London, Oxford, Paris, Frankfurt, Weisbaden, Oeynhausen, Minden, Essen, Cologne, Bonn, Berlin, Heidelberg, Stuttgart, Nuremberg, Munich, Vienna, Salzburg, Prague, Stockholm, Amsterdam, The Hague, and Brussels. The book is a travelogue, marked by observations and comments typical of the first-time visitor (which Bailey was), but it is much more than that. According to Bailey, it is a "personalized report" (p. 241), "a primary source" and thus "an historical document of some value" (p. vii). It is, in fact, the recorded impressions and observations of an experienced and successful diplomatic historian who saw a great many people and a great deal of Europe during the summer when many people were approaching starvation and several nations were reaching the point of economic collapse as a result of five years of war and two additional years of postwar economic deterioration.

Bailey has taken the trouble to check his notes and other materials against monographs and documents that have been published since his visit in 1947. From these he sharpened his recollections and gleaned additional detail. Particularly noteworthy are the repeated references to the *Foreign Relations of the United States*, which Bailey used with great effect to enrich his account. Also indicative of a good deal of reaching beyond the bare travel record he kept are Bailey's historical vignettes. For example, he commented on the war crimes trials when he described his visit to Nuremberg (pp. 132–36). He wrote a little essay on Harold Laski, whom he interviewed in England (pp. 211–16). And he commented on the black market in Germany when he described his visit with the American education officer in Berlin (pp. 101–104). Fleshing-out thirty-year-old notes in this way is most helpful, but it can also lead to surprises: the exchange rate Bailey gives for German marks in 1947 is inaccurate (p. 101). It is also revealing to learn that the American education officer in Berlin engaged in illegal black market activities to help put students through the University of Berlin.

Bailey's contacts in Europe were for the most part Americans or people he met by arrangement through Americans in the embassies or in the military governments of Germany and Austria. One as thoroughly trained in the evaluation of evidence as Bailey has a mind of his own to judge the authenticity of what he sees and hears. But one nevertheless wonders whether some of his impressions and observations were influenced by the people he met and talked with and by the purposes of those who offered him the trip as an inducement in the first place. Wherever he went in Europe, Bailey records that he

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heard about the awful Russians and their ominous intentions. Significantly, in discussing why he skipped over Warsaw on his trip from Prague to Stockholm, he writes that "not much new could be learned there [in Warsaw] about secret Soviet designs" (p. 183). In any case, I find it incredible that Bailey spent as much time as he did in Germany without ever recording a criticism of French policies and actions there. George Meader heard such things when he went to Germany for a Senate committee late in 1946, and so did Walt W. Rostow (though he chose not to believe them) when he went there for the State Department in the summer of 1946. Perhaps things had changed by 1947.

JOHN GIMBEL
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The Soviet Union and International Oil Politics *by Arthur Klinghoffer. New York, Columbia University Press, 1977. 389 pp. $16.50.*

A few examples selected at random from recent news reports suggest the growing importance of oil as a factor in the Soviet Union's domestic and foreign policy decision making. Item: An Arabic language weekly publishes documentary evidence that as early as 1967 Premier Kosygin urged Arab leaders to use the oil weapon to "split the ranks of the imperialists" and "create political problems" for the United States and its NATO allies. Item: The Saudi oil minister warns that the Kremlin is seeking geopolitical influence and strategic strongholds in Africa and the Middle East with the ultimate aim of threatening Western oil supplies and shipping routes. Item: U.S. government and business experts predict that a looming Soviet energy shortage will place severe constraints on an economy already troubled by declining growth rates. Item: The Soviet press launches a campaign against government ministries responsible for production lags in the west Siberian oil fields that must provide most of the oil needed during the next decade.

Professor Klinghoffer's timely and extensively researched book is one of the first *political* analyses of Soviet oil policies. The author's main purpose is to document how Moscow has used oil to further its foreign policy goals, but his perspective does not blind him to the economic aspects of Soviet oil policies. Indeed, his case studies could be divided into those in which the political component is uppermost and those in which economic considerations are dominant.

The oil weapon is more important in Soviet relations with allies than adversaries. Klinghoffer argues that the Kremlin uses its control over the oil supply of most of the communist-ruled states to keep them within its sphere of influence and on occasion has tried to reign in the mavericks that have sought to free themselves from Soviet domination. This tactic has not, however, been very successful. The cut off of oil shipments to Yugoslavia and China and the withdrawal of financial and technical aid to Albania's oil industry did not prevent those states from leaving the Soviet bloc. I would argue, however, that Soviet oil exports to Eastern Europe are used more directly for economic, rather than political, effect and less for outright political control than for the USSR's own political protection. By providing the energy-poor bloc countries with most of their oil, Moscow's most immediate concern is to shore up their domestic economies upon which,

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however, their internal political stability and reliability—and Soviet security and ideological interests—ultimately depend.

Western Europe is the largest importer of Soviet oil outside Comecon. Soviet-West European oil trade is based entirely upon commercial considerations, and Soviet exports generate badly needed hard currency and Western technology and equipment. (Approximately half of the USSR's hard currency earnings for 1977 was derived from oil sales.)

There is a political motive behind exports to certain Third World countries in which Moscow has attempted to reduce Western influence and generate support for Soviet interests by offering an alternative to Western-supplied oil. But this does not represent a major effort, since only a small portion of Soviet oil exports are reserved for this market, and the Soviet Union has been far more successful in using other means, for example, arms sales, to gain footholds in the Third World.

Not surprisingly, Soviet oil policies in the Middle East and North Africa are complex and multifaceted. As part of their overall effort to reduce Western influence, the Soviets have encouraged such anti-Western actions as embargoes and nationalization of Western oil properties in the belief that the West's control over oil in the region is the key to its strong political and economic position there. The Soviet Union imports oil from the region for good economic reasons; it is cheaper to buy oil in the Middle East and ship it to European Russia than it is to produce oil in Siberia and transport it to the USSR's urban and industrial centers. (It has been estimated that a barrel of Siberian oil costs a little more than twice the price of a barrel of OPEC oil.)

Sometimes Moscow manages to have it both ways, serving its political and economic interests at the same time. One of the most intriguing ironies uncovered by Klinghoffer is that the Soviets encouraged the Arab oil embargo of 1973–74 while making a tidy profit on oil sold to two of the main targets of that embargo—the United States and the Netherlands.

Klinghoffer's book is a valuable survey of Soviet oil trade policy during a period in which oil production programs were adequate for simultaneously meeting domestic needs, providing a safety net for other Comecon countries, and exporting substantial quantities of oil. That period, however, appears to be drawing to a close. Klinghoffer predicts that the Soviet's will probably be able to sustain oil production growth rates necessary to meet most of their domestic and foreign goals, but a number of recent studies suggest that Soviet oil production will actually decline over the next few years—drastically so if new oil fields are not located and developed rapidly enough. Even the Soviet press has begun to discuss candidly some of the bureaucratic, technological, financial, and geographical problems behind current production lags in the high-priority region of western Siberia. If, as some energy experts predict, the USSR has difficulty in just meeting its domestic energy needs in the 1980s, this will have very serious political and economic implications with regard to Eastern Europe and Soviet hard currency earnings in the West.

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* The opinions expressed in this review are those of the author and do not necessarily reflect the views of the Central Intelligence Agency.

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Conflict Among Nations: Bargaining, Decision Making, and System Structure in International Crises *by Glenn Snyder and Paul Diesing. Princeton, N.J., Princeton University Press, 1977. 570 pp. Cloth, $32.50; paper, $9.50.*

This book attempts to develop a theory of international crisis behavior using theories of bargaining, international system structures, and decision making. It is an exceptionally ambitious work both in the breadth of theoretical approaches explored and the span of history covered. The data are drawn from 16 case studies of international crises from 1898 to 1973. An introductory chapter defines the scope of the work, explains how the authors conceive of international crises, and briefly presents the three theories to be explored. Chapter two presents several formal conceptions of bargaining, including utility models that focus on the utilities or goods that are allocated during bargaining and models of strategic interaction, which are game models that focus on general strategies available to the players. The authors' exhaustive analysis of numerous types of game models will delight game theory buffs.

Chapter three presents a less formal model of strategies and tactics in bargaining, concentrating on value structures involved in crises. The authors explore the nature of "bargaining power" and analyze specific strategies and tactics in the bargaining process. Chapter four concerns information processing and, contrasted to the previous chapter, concentrates on the "process of correcting initial errors of estimation and interpretation" (p. 282). Chapter five examines three decision-making theories; namely, utility or rational actor theory, bounded rationality, and bureaucratic politics. In chapter six, the authors investigate the effect that international systems—bipolar or multipolar—have on the bargaining process. Chapter seven purports to present a summary and synthesis of the theory of crisis behavior.

On the positive side of the ledger, the authors have endeavored to accomplish what everyone in international relations agrees should be our goal, but few ever attempt; namely, the combination of various levels of theory. The authors are thus correct in contending that their analysis is extensive, both theoretically and historically. It is unfortunate that their "synthesis" never quite emerges in the way the reader is led to expect, namely showing the combined and interrelating effects of bargaining, decision making, and system structure on crisis behavior. The authors' attempt is admirable, and their failure reflects the difficulty of the task.

Also troublesome, given the authors' stated purposes and claims, is the failure to test the theories. Their "tests" primarily consist of illustrations of theoretical propositions by reference to one or more of their cases. The authors do not mislead in this regard, for despite their use of the term "test," they make it clear that they are not interested in predictions as much as in "fruitfulness and relevance" (p. 67). Nevertheless, readers looking for systematic tests of propositions generated by three theories will be disappointed. A related observation concerns the assessment of exactly how the authors came to some of their conclusions, for procedures for evaluating evidence are not always spelled out.

Yet another methodological problem, common to many bargaining studies, is the imputation of motives to decision makers. For example, the authors note that any interpretation of the Quemoy crisis of 1958 is "hazardous" because of the lack of data about motivations and decision making, especially on the Chinese side. Still, they go on to

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argue that "the pattern mentioned above shows up quite clearly, however. No doubt the Chinese Communists were worried about Chiang's concentration of forces on Quemoy, worried that. . . " (p. 258). In almost all cases, the "evidence" on motives comes from inferences based on how the decision makers acted in the crisis; while the interpretations are plausible, the "evidence" is just that—plausible—and is thus less concrete than the authors imply and much less objective than one needs to "test" theories.

Several problems can be noted of a somewhat more cosmetic nature. Arguments are sometimes presented that the authors imply are either novel or generated from their cases, but which in reality are fairly common assertions already contained in the literature on bargaining processes (see, in particular, pp. 10, 38, 71, 97, and 334). In addition, portions of the book are tedious. The lengthy descriptions of numerous games and models, the multiple options open to bargainers in crisis situations, the long descriptions of minute aspects of one or two cases—all of these scattered throughout the book make for laborious reading. A copyeditor with a sharp pencil would have been helpful. Clear summary statements at the end of sections and chapters are also needed. Without these, the reader must work to place the discussion in context.

The importance of the flaws noted here will depend of course on the goals of the individual reader. These flaws would be truly devastating if one could suggest to the authors alternate methods or procedures that would correct them. I cannot and doubt that many others can either. Thus the book clearly stands as an ambitious effort to assess the importance of three different islands of theory for understanding crisis behavior. The authors have spelled out the three approaches very clearly, have exhaustively utilized history to illustrate components of the three theories, and have moved us somewhat along the way toward a more complex understanding of crisis behavior. The fact that the "synthesis" of the theories does not quite come off does not detract fatally from their ambitious effort.

MICHAEL P. SULLIVAN
University of Arizona

Command Decision and the Presidency: A Study in National Security Policy and Organization *by R. Gordon Hoxie. New York, Reader's Digest Press, 1977. 505 pp. $15.00.*

R. Gordon Hoxie's study purports to review the postwar evolution of national security policy and organization and the central role of the president, as commander-in-chief, in this development. This claim, reflected in the title, is exaggerated. Much of the discussion is devoted to the Truman and Eisenhower presidencies. The security policies and processes of decision of the other presidents since World War II, from Kennedy through Carter, are given short shrift. The experience of the post-Eisenhower presidencies is treated selectively and largely as a foil to highlight or contrast the handling of national security problems by Truman and Eisenhower. They are the heroes of the piece. Truman is portrayed in familiar tones as shrewd, tough, combative, and decisive. Eisenhower is painted in lighter pastels as modest, equable, prudent, and judicious. The book is in

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great part more a vehicle for Hoxie's expression of personal likes and dislikes of postwar presidents than a systematic study of national security. We learn more about his political predilections than about the strengths and weaknesses of American national security policies and decision-making processes.

Difficulties start early with the title of the book. Command decisions are nowhere clearly defined. They are identified with Truman's firing of MacArthur, Kennedy's fiasco in the Bay of Pigs, Johnson's pursuit of a nonproliferation treaty, and Nixon's overture to China. Except for economic issues, like trade and monetary policy, the reader is left to conclude that all other presidential activities affecting other states and governments are command decisions.

Hoxie's lack of sufficient reflection about what he is about has at least two unfortunate consequences. First, material that should properly be included is ignored or superficially treated. Lost from sight is the development of the National Security Council as an institution and its transformation under succeeding presidents, the role of the president's chief adviser for national security (Kissinger is hardly mentioned), and the relation of presidential advisers to the secretaries of State, Defense, and the CIA and their staffs. Relations with Congress, though repeatedly termed critical to effective national security, are nowhere thoroughly explored. It is as if the Admiral's Revolt of 1949, the attack on the New Look in the 1950s, the so-called missile gap debate, the quarrel between Kennedy and Carl Vinson over the RS-70 and sundry other military matters, the gradual erosion of congressional support for the Vietnam War, the ABM debate, and the battle over passage of SALT I in the Senate had never occurred on the legislative battleground.

What is discussed is depoliticized. The president acts in a political vacuum. In Hoxie's presentation presidential decisions and behavior occur outside the play of strong personalities, driven by competing ambitions, attached to powerful political interests, and bent on prevailing on vital national issues. The insights, however limited, of the bureaucratic politics literature are not utilized in a study that is totally dependent on secondary sources for its material. Since politics has no standing in Hoxie's conception of the idealized presidency, the men who occupy the post and direct the fate of the institution appear more as lifeless monuments, like the statues of generals in public parks, than as actors impressing themselves on their roles and on other men, government institutions, and states.

These criticisms are not made without regret since Hoxie has displayed rare entrepreneurial skills in promoting the study of the presidency. His book is obviously a labor of love, but love unrequited for the reader and serious student of the presidency.

EDWARD A. KOLODZIEJ
University of Illinois

Can Business Management Save the Cities? *by David Rogers. New York, The Free Press, 1978. 276 pp. $13.95.*

There are many ways to apply "business management" to cities: management training for city officials, either before or after they assume their positions; interchange of person-

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nel between business and government; use of the private sector to deliver public services, via contracts; increased reliance on the use of consultants, paid or voluntary; and organized civic efforts by the local business community. David Rogers examines in detail a case study of one important and large-scale effort in New York City—the experiences of the Economic Development Council (EDC), a nonprofit organization with almost 200 New York corporations as members. EDC functioned principally as a voluntary management consulting firm; it recruited executives, on loan from its member corporations, to work on EDC task forces in city agencies at no cost to the city.

In the first two chapters Rogers sets forth, concisely and perceptively, the theoretical basis for the problems of managing in city governments and contrasts the managerial environment in the public and private sectors. His is not a simplistic, mechanistic, technocratic view; he is keenly aware of the role of political rationality and the fact that it often outweighs economic rationality.

After describing EDC, its origins, composition, and activities, the book describes and evaluates EDC's experiment in management transfer as a way to improve New York City's management. It focuses on three specific projects: in school headquarters, in high schools, and in the courts.

The projects were conducted by EDC task forces directed by retired top executives and staffed by younger executives on loan—a formidable array of talent. EDC's role, in retrospect, was to support innovation-minded insiders in the client agencies, to act as a third-party mediator between agency factions, and to bring in new resources. Much was accomplished but much remained to be done. Rogers concludes that the EDC approach has much to commend it and should be supported, in New York and elsewhere, despite the complexities involved in carrying it out.

To be successful, such projects require the following preconditions on the part of the agency: the perception by top agency officials that their agency faces a crisis; agency officials with the power and commitment to make reforms; support of these reforms by a core of people at various levels in the agency; not too large a setting. The projects that were most successful, according to Rogers's analysis, were those that: were well defined; had identifiable and measurable goals; were relatively short term; operated in limited parts of the agency; dealt with technical, managerial functions that were not enmeshed in political controversy; and exhibited a good fit between agency and EDC personnel.

The dilemmas facing such efforts are the following: (1) co-optation vs. confrontation; that is, one has to be collaborative in style to gain entry, information, and confidence, but one has to be aggressive in pushing for acceptance and implementation of recommendations for change; (2) how to retain corporate support in the face of limited results; and (3) the entire strategy focuses on administrative improvements when what is often needed is a comprehensive and fundamental policy change. I concur with this last point and believe, for example, that significant improvements in the New York school system are more likely to be brought about by a voucher system, extensive decentralization, and tuition credits—that together strengthen private schools and offer competition to the public education monopoly—than by administrative reforms in school headquarters.

Rogers believes that the EDC approach would be even more successful in smaller cities, and his book can confidently be recommended to business and civic groups that want

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to improve management in local government. Such efforts are modest in their impact, but then what isn't in this complicated setting?

EMANUEL S. SAVAS
Columbia University

Decision to Prosecute: Organization and Public Policy in the Antitrust Division

by Suzanne Weaver. Cambridge, Mass., The MIT Press, 1977. 196 pp. $14.95.

This fine book uses a case study of decision making in the Justice Department's antitrust division to develop both empirical and general analyses which are important to several audiences: economists, lawyers, and political scientists interested in government intervention in the economy, students of public and private organizations, and political scientists trying to overcome the narrowness of the public law field. Professor Weaver's central focus is how the division selects cases to prosecute. But in answering this, she enriches our understanding of larger issues such as the nature and limits of courts and lawyers as policy makers; the exercise of administrative discretion; the tension between the need for an independent prosecutor's office and the need for control and accountability of agencies by elected officials; alternatives to government economic intervention through regulatory commissions.

We increasingly turn to courts to make policy as well as to adjudicate conflicts. And outside of the courts many agencies are dominated by lawyers. This study indicates the nature and limits of courts and lawyers as policy makers. Cases are chosen on ad hoc, particularistic bases under a prosecutorial ethos of whether the case at hand can be won and will thus maintain the division's professional reputation. Alternative economic criteria, such as the actual economic impact of alleged anticompetitive behavior, are eschewed. (Weaver chronicles the problems and ultimate failure of a recent division head who sought to introduce greater use of economic criteria.) The division's enforcement policy is almost exclusively court-oriented. Since the division's lawyers use current antitrust law as a major guide for future prosecutions and since the law is largely what the courts say it is (legislative standards are extremely vague), this policy-making process is self-reinforcing.

The antitrust division's reliance on the courts as the final arbiter of its decisions has significant consequences: Most of the agency's work is done by lawyers rather than economists; the lawyers feel that, since courts make the final decision as to what antitrust policy will be, they are free from the obligation to take a neutral and objective view of the division's decisions. They are free to be advocates. They seize whatever is helpful, discard whatever might weigh against them; they gather evidence rather than facts; they are brief-writers rather than truth-seekers. They do this secure in the knowledge that policy is being made in an adversary process in which the other side is taking the same approach and in which the final decision will not be made by either adversary, but by a neutral arbiter—the judge.

The benefits of an adversary process in the adjudication of conflicts have been persuasively shown elsewhere. But an equally persuasive case can be made that this process

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has serious limits for making policy in complex areas. The division's adversary role and reliance on the courts as final arbiter allows it to be free from the responsibility of developing a comprehensive view of the merits of possible antitrust cases. The division mounts large antitrust campaigns but it does not systematically focus on the most economically significant matters: "It has pursued the cases at hand, and pursued the general idea of extending the reach of the law, without consistent regard for a coherent notion of economic competition and of the division's role in promoting it" (p. 177).

The limits of this adversary process seem to apply to other areas in which lawyers are policy makers or even analysts such as the work of Nader and other public interest legal groups, the FTC, and recent proposals for "science courts" to resolve scientific policy controversies.

Weaver also indicates some of the perplexing tensions in economic regulation and public bureaucracies in general. For instance, she found that here, as in other policy areas, there are important limits to executive control over subordinate agencies. The force of professional norms is especially important in this. Thus, on the one hand, the divisions' emphasis on professional norms (the values of legal craftsmanship and professional peer acceptance) and a prosecutorial ethos (a high conviction rate) creates a rather narrow conception of its task and more narrow policy making than might result from other approaches. On the other hand, this emphasis is also responsible for the division's insulation from external influences: the White House, the Attorney General, and even the division head have had limited ability to alter the division's policies or even procedures (including the relative inability of Thurman Arnold to do this). This and parallel situations with independent special prosecutors raise serious questions about the needs of democratic control and accountability relative to the needs of independent policy making.

Another policy dilemma is that although independent regulatory commissions have been criticized as being co-opted by the businesses under their jurisdiction and being open to intrusions of partisan politics, Weaver found that the antitrust division is almost completely free of these characteristics. However, the very factors that prevent them from developing, such as a reliance on the courts as final arbiter, lead to some serious disadvantages for economic policy making. One of these is the division's relative irresponsibility about developing a substantive rather than prosecutorial view of what antitrust policy is. For this reason and others, Weaver wisely suggests caution in following the current regulatory reformers' enthusiasm for the adversarial approach.

MARTIN A. LEVIN
Brandeis University

Counsel for the United States: U.S. Attorneys in the Political and Legal Systems

by James Eisenstein. Baltimore, The Johns Hopkins University Press, 1978. 264 pp. $15.00.

Reading Eisenstein's book was for me a troubling experience. It is, I think, a good book, a book by a skilled social scientist carrying out in a not unsophisticated way the standard operations of our enterprise. But, perhaps its mainstream quality, its very goodness as

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standard social science, is what troubled me. Here is a book about lawyers, a book about lawyers formally appended to and charged with acting for the government of the United States; here is a book which pays almost no attention to the special activities of lawyering. Eisenstein has, in essence, written two "books," one much richer than the other and one which emerges only as the other unfolds.

The first book is simply an analysis of patterns of interaction among United States attorneys and the other actors in what Eisenstein calls their "strategic environment." The second and far more interesting book takes as its subject the entire federal legal system and seeks, albeit with modesty, to analyze that system from the perspective of United States attorneys. The first book sticks closely to theories derived from public administration and role theory; the second is, although only implicitly so, anthropological. The former views the activities of United States attorneys through the eyes of a social scientist, the latter describes and discusses the procedures, policies, and rituals of the federal legal system with the eyes of an important participant in those procedures and rituals and an important maker of those policies.

In neither of his books does Eisenstein penetrate to the distinctive activities of lawyering that constitute the work of United States attorneys. There are but hints of an understanding of what the prosecution and defense of cases is all about, of what it means to make judgments, legal judgments, about human behavior, of the way in which legal norms impinge upon and shape individual perceptions and official behavior. There is, in neither of Eisensteins books, a view of the way cases develop or how prosecutors go about making cases. We get too little about the actual activities of prosecution and defense and too much about the relations of United States attorneys and their significant others.

Eisenstein's first book takes as its central question the extent to which United States attorneys may be said to be autonomous in their relations with their administrative superiors in the Department of Justice, judges with whom they work in the federal district courts, investigative agencies, and members of the private bar. It highlights varying patterns of control and independence, varying patterns of cooperation and conflict all of which appear to be characteristic of such relations. United States attorneys appear to be, in general if not uniformly, more dependent upon and embedded in the cultures and mores of their individual districts than dependent upon and embedded in the formal administrative structure of the Department of Justice. Their relation to Justice, that is, their relative autonomy is, according to Eisenstein, largely dependent upon the size of their office. The larger the office, the more it takes on a bureaucratic structure which allows for effective resistance to outside pressures.

Eisenstein demonstrates that United States attorneys may be more or less autonomous, yet he never establishes why autonomy ought to be a central concern. Perhaps the relative autonomy of United States attorneys determines the extent to which uniformity will be achieved in the policies of the Department of Justice or the extent to which equal treatment will be provided from one district to another. Perhaps autonomy prevents the colonization of the federal legal system by partisan politics. Perhaps autonomy or the lack thereof may be most important as a determinant of whether United States attorneys can and do adhere to formal legal norms governing their activities. The autonomy of these lawyers may have as a parallel the independence of the judiciary, and it may have a similar rationale, namely, to guarantee the separateness of law and politics and the

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distinctiveness of lawyering even when such lawyering takes place in the service of the state. What Eisenstein demonstrates is that the autonomy of United States attorneys is much more precarious and uncertain than is the independence of judges.

Eisenstein's second book indicates that the operation of the federal legal system is similar but not identical to the operation of legal systems in the various states. Patterns of interaction in the federal system are considerably different from those described in Eisenstein's work on criminal justice at the state level (*Felony Justice*). From the perspective of the United States attorney that system is orderly and patterned. Caseloads are significantly lower than those that burden state systems. Prosecutors have substantial control over investigative agencies and thus are in a good position to structure the entire criminal process. This control is itself a significant resource that must affect the way in which prosecution occurs. Eisenstein regretably does not explore this linkage. He does, however, supply interesting insights into a variety of processes at the federal level that have received little attention, the most interesting of which concerns plea bargaining.

Plea bargaining is, according to Eisenstein, as prevalent at the federal as it is at the state level. However, it is more restricted. It generally takes the form of negotiations concerning the number of counts in an indictment rather than the reduction of charges or sentences. It is restricted because federal prosecutors are typically able to make stronger cases or are, at least, more likely to decline to prosecute weaker cases. But what does it mean to talk about stronger cases? Does it mean that there is a greater concern for formal legality at the federal than at the state level? Does it mean that United States attorneys act as independent judges of the virtues of prosecuting individual cases rather than as the subservient agents of a repressive political order? Eisenstein's work raises, again implicitly, such questions, but his concern as well as his own inclinations lead him away from rather than toward them.

This is a hard book to read. It suffers from a constraining symmetry in presentation. That symmetry stands in the stead of any unifying argument. Yet, as difficult as it may be to read, the book is a pioneering effort. It pioneers in focusing attention on United States attorneys as key actors in the legal system. Yet, it is far from pioneering in its approach or insight. It carries forward uncritically the tradition of legal realism; it does not illuminate or expand that tradition nor does it question it. Eisenstein's work demonstrates that lawyers can be understood within the frame of standard social science theory, but it leaves nagging doubt that such an understanding is either sufficiently rich or finally satisfying.

AUSTIN SARAT
Amherst College

The Investigators: Managing FBI and Narcotics Agents *by James Q. Wilson. New York, Basic Books, 1978. 228 pp. $10.00.*

One of the foremost students of public administration, James Wilson proposes in this brief study to survey the "relationship between the tasks" of the FBI and the Drug Enforcement Administration (DEA) and the "organizational structures within which those

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tasks are performed" as the basis for appraising the "consequences of practicing *public* administration" (p. ix). Exploring in detail the investigative activities of FBI and DEA agents, how managers attempt to control and direct agent investigations, and the administrative priorities of FBI and DEA executives, Wilson concludes that "to a substantial degree, the administrative systems of the [FBI and DEA] have been shaped by the political maintenance needs of the two organizations rather than by the nature of the tasks the organizations' operators perform" (p. 192). Elaborating on this point, Wilson posits that "the contraints [on the FBI and DEA] arise . . . out of the fact that these are *governmental* agencies, and especially out of the fact that they function in the particular constitutional and political arrangements of American government" (p. 196).

These are unqualified and, to an extent, surprising assertions. Supporting conservative conceptions of federal regulatory policy, Wilson's conclusions also indirectly challenge the conclusions of other students of the FBI who have stressed either that the bureau's abuses of power derived from independence or the highly politicized nature of FBI investigations. Are these then principally differences of interpretation? Does Wilson's research and analysis support his contrary contentions?

Unfortunately, Wilson's contentions are not convincingly documented. This is due primarily to the limited focus of *The Investigators.* In rationalizing his decision to delimit his analysis to FBI criminal investigations, Wilson maintains "I do not discuss in detail other tasks some agents perform, such as the domestic security and counterintelligence investigations carried out by the FBI. . . . The reason for emphasizing criminal investigation is that this is what I could study, this task consumes the bulk of each organization's resources . . . and this work will continue to be performed by these agencies whatever the fate of proposals to curtail or assign to other organizations counterintelligence, security, and regulatory functions" (p. 8).

This delimitation decision might have been reasonable five years ago. However, FBI domestic security and counterintelligence investigations can now be studied: FBI and DEA policy documents have been declassified (notably by the Church Committee and the House Subcommittee on Government Information and Individual Rights) and are available for scholarly examination. Although criminal investigations comprise the bulk of FBI agents' time (in number of cases), domestic security and counterintelligence investigations reflected FBI officials' investigative priorities and indirectly determined how FBI time and resources were apportioned to other investigations. Domestic security investigations, in part because they were not predicated on statutory authority, are more interesting to the student of public administration. Moreover, some domestic security and counterintelligence investigations (the infamous COINTELPROS and the emergency detention programs) confirm the independence—even insubordination—and the political activism of FBI officials; others (notably, separate filing and record destruction procedures such as the "Do Not File" procedure for break ins) demonstrate a disdain for legal and constitutional prohibitions. In sum, these investigative activities challenge Wilson's contentions about the "consequences of practicing *public* administration" and how governmental agencies are constrained by the "constitutional and political arrangements of American government."

Were these typical or atypical practices? Do they instead confirm some scholars' view of the FBI as an independent and purposeful bureaucracy? Were these activities peculiar

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only to the FBI or peculiar to the FBI's operation during the height of the cold war years? Because Wilson intentionally restricted the scope of his research, these challenging and important questions are not addressed. Perhaps these striking deficiencies of *The Investigators* will impel other students of public administration to initiate this more ambitious and needed study.

ATHAN THEOHARIS
Marquette University

Stability, Security, and Continuity: Mr. Justice Burton and Decision-Making in the Supreme Court 1945–1958 *by Mary Frances Berry. Westport, Conn., Greenwood Press, 1978. 286 pp. $17.50.*

This study makes the most extensive use of the Harold H. Burton papers in the Library of Congress thus far published. The approach throughout is measured, usually descriptive rather than evaluative, and sympathetic toward those particular strengths—perhaps best synthesized by that old-fashioned, but indispensable, word "character"—that Justice Burton brought to the Supreme Court. Professor Berry has reconstructed from Justice Burton's notes on what was said and done a valuable and skillfully blended resumé of what went on inside the Supreme Court's private conferences over a period of thirteen turbulent years.

As the title indicates, this is not a traditional biography of a public man. It is instead a review of Justice Burton's participation in those cases for which Supreme Court papers are available. The Burton papers contain brief citations of viewpoints expressed by the various justices as discussion proceeded within the weekly conferences. There are, of course, some risks in relying exclusively on this kind of material, interesting as it is. There is an unexamined assumption that the observer (Burton) is always accurate in his reportage. There is the further, and related, assumption that the researcher is correctly construing the various clauses and half-sentences that were hurriedly jotted down for the participant's own benefit. A classic illustration of the problem can be found in the comment attributed to Justice Hugo Black during one of the discussions of the Desegregation Cases. "Black was 'compelled' to comment that the reason behind segregation was the belief that black people were 'inferior,' to which was added the fear of the possible 'mixture of races,' for 'it [the mixture] weakens the white race.' However, he would vote with a majority to reverse" (p. 124). This kind of talk requires explanation; the explanation offered is that Justice Black was explaining to his non-Southern colleagues something about a mentality with which he was more familiar than the others, including Justice Reed (Kentucky) and Justice Clark (Texas). We know a good deal about Justice Black's opposition to segregation, yet an unwary reader might unfairly construe these isolated phrases to Black's disadvantage. In fact, his son, Hugo Black Jr., reports in his candid memoir about his father that the justice was exceedingly upset by the way in which snippets from Court papers concerning his role in the Desegregaton Cases had been put to use by several scholars. I do not suggest that Professor Berry has misrepresented Justice Black's views; however, these kinds of materials upon occasion need added exegesis, else we become a victim of hearsay testimony of the most misleading sort.

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Justice Felix Frankfurter, who interacted continuously with Burton, does not emerge as a sympathetic figure. He is seen as fussy, argumentative to a fault, and concerned less with deciding cases than confronting procedural and process issues of mixed importance. Mention is made of the vote the law clerks took one term in which it was determined that Justice Burton was the model Supreme Court justice. Only one or perhaps two clerks dissented, including a Burton clerk who was an admirer of Frankfurter. It may be added (and I have this from the clerk himself) that even at the time of the vote he humorously acknowledged to himself that "one or two Frankfurters are great, but nine of them would be a few Frankfurters too many!" I suspect our author may agree.

A few small matters escaped detection: Chessman was not the Birdman of Alcatraz (p. 206); nor did Justice Burton write his interesting articles on Supreme Court history unassisted by his law clerks in every case (p. 267). Nonetheless, overall this is a welcome addition to the literature on the Supreme Court.

DAVID N. ATKINSON
University of Missouri, Kansas City

The Case-or-Controversy Provision *by James E. Radcliffe. University Park, The Pennsylvania State University Press, 1978. 285 pp. $13.50.*

Section 2 of Article III of the U.S. Constitution limits the federal judicial power to *cases* or *controversies*. But these terms are not defined in the document, and their meaning cannot be discovered by examining the intent of those who wrote it. In the last analysis, a case is a proper case, suitable for judicial action, if the judges so hold, and in our system, the U.S. Supreme Court speaks the last word as to what the requirements of a case really are. This learned book is a first attempt by a political science scholar to summarize in an orderly and analytical way how the Supreme Court has applied the case-or-controversy provision in the course of its history. This is an extremely important subject, but it is also highly complex, and in the words of the author, "elusive and imprecise." As a matter of fact, Dr. Radcliffe, who is a professor of government at Shippensburg State College, makes it clear that the Court paid very little attention to case-or-controversy problems until about 1880, and in recent years has been especially active in adjusting relevant doctine to the requirements of public law litigation. Perhaps the most important general point made by the author is the distinction between private litigation and public law litigation, and while the Court has never actually said so, it has been fashioning rules for the latter not entirely applicable to the former.

This book reviews the decisions of the Supreme Court on a wide variety of topics, many of which are old stand-bys in American constitutional law. Summarizing the history of the provision, Dr. Radcliffe concludes a historical review with the observation that its "established requirements include adversity, parties with an interest in a disputed legal right, the existence of an actual justiciable controversy, and a federal court's ability to render a final and binding judgment" (p. 45). Thus, the succeeding chapters deal with the following topics: adversity ("the most generally accepted element of a 'case' or 'controversy' and . . . one the Court has always overtly required"); sources of legal rights and

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remedies (the Constitution, statutory law, treaties, and state law, including common law and equity); the existence of a sufficient interest (substantiality, directness, personal quality, public interest); existence of an actual justiciable controversy (types of action, types of issues); finality (judgments subject to executive or legislative review, execution of judgments); and doctrines of justiciability (advisory opinions, standing, mootness, political questions).

The author makes it perfectly clear that all of these case-or-controversy doctrines are vague and flexible, and accordingly, when the Court wants to decide a case, it is not too difficult to figure out a rationale to establish its legitimacy as a proper case. Thus, the author observes, in concluding his chapter on the sufficiency of interest of the parties, that "when the Court wishes to become an active participant in the political process, it has little difficulty in determining that a party has a sufficient interest" (p. 125). And, speaking generally, the author believes that the Court has been successful in adapting doctrines long appropriate for private litigation to the requirements of public law litigation, so that the Court may be able to accomplish its political as well as its strictly legal duties to our society.

The author is to be complimented for his courage in tackling such a technical legal subject. At the same time, in seeking to review so many decisions, he makes an occasional error, usually of omission. For example, in discussing *Barlow* v. *Collins* (1970), in which the Court permitted tenant farmers to challenge regulations promulgated by the secretary of agriculture, the author fails to tell the reader enough to understand what the dispute was all about (p. 115). The point was that the farmers in question were seeking to set aside rules which enlarged their scope of personal freedom of action, thus presenting the question as to whether they had a sufficient interest to have standing to sue. The usual rule is that one cannot challenge rules which are favorable to his interests. Furthermore, one of the hazards of writing on legal subjects is that the law is in a process of almost continuous change, and those who write on the law must be alert to take note of the changes. Thus, there is a reference to the use of three-judge federal district courts in cases involving the validity of state laws, the footnote reference being to 28 U.S.C. § 2281 (p. 161). Actually, this section was repealed by Congress with The Three-Judge Court Amendments Act of August 12, 1976 (Public Law 94-381), which abolished three-judge courts for cases involving the validity of federal or state statutes, and retained that device for only a few situations specifically required by acts of Congress (such as actions involving the constitutionality of the apportionment of congressional districts or of any statewide legislative body). Finally, to cite one more example, the discussion of the diversity of jurisdiction of the federal district courts leaves much to be desired (pp. 83–4). For one thing, the Rules of Decision statute does not "authorize" the federal courts to follow state law in diversity cases; it *requires* them to do so. For another, the *Erie* decision did not, as suggested, obliterate the distinction between substantive and procedural law; on the contrary, as Justice Brandeis himself indicated in the *Erie* opinion, the federal district courts are not obliged to follow state procedural law in diversity cases, and the big argument since then has been over where to draw the line between substantive and procedural law.

One may suppose that one can always find points at which to carp in a book concerned with such a technical and complex body of imprecise legal doctrine. On the whole,

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Radcliffe has made an extremely valuable contribution to the literature of American constitutional law. The book merits the attention of all who work in the field.

DAVID FELLMAN
University of Wisconsin-Madison

State Policies and Federal Programs: Priorities and Constraints *by Peter Passell and Leonard Ross. New York, Praeger Publishers, 1978. 168 pp. $16.95.*

This Twentieth Century Fund report describes current problems in five policy areas—fiscal affairs, school finance, higher education, health care, and income maintenance—with special emphasis on intergovernmental relations. Standard reform proposals are discussed in each area—again with special reference to their potential impact on intergovernmental relations.

There are no original proposals presented in this volume; no significant reformulations of national problem areas; no genuine insights; no systematic research on the topics discussed. Scholars who are familiar with the fields under study will find little to interest them in this general review of domestic policy by two professional writers. Nonetheless, the issues are clearly presented; the prose is readable; and each topic is discussed in a balanced and knowledgeable fashion.

Passell and Ross express a concern at the outset for "maintaining an integrated private economy with free movement of individuals" (p. 32) and retaining the efficiency of federalism and decentralized government. However, the authors are even more concerned with "national responsibility to provide reasonable living standards and economic security for everyone" (p. 32). They suggest that the best approach to balancing conflicting values is a pragmatic evaluation of "how individual programs work in practice."

After describing current programs and proposed reforms in "fiscal federalism" (intergovernmental revenue transfers), elementary and secondary education, higher education, health care, and income maintenance, the authors endorse reforms that call for greater federal funding and increased federal administrative intervention. The values of federalism are apparently outweighed by the values of redistributive justice, programmatic uniformity, and the implementation of national priorities over local or regional preferences.

Specifically the authors call for: an expansion of federal revenue sharing with a distribution formula that rewards progressive taxation in the states; increased federal aid to education with requirements that states achieve greater intrastate equalization of school funding; comprehensive national health insurance coupled with improved delivery systems and controls on cost; and "fundamental reform" of the nation's income maintenance system involving a "transfer of the political risks to federal officeholders."

There is nothing wrong with these prescriptions—but they are commonplace recommendations and this book adds little to our understanding of their potential impact. Indeed, this book is little more than a rehash of standard textbook material on domestic public policy with standard calls for reform.

THOMAS R. DYE
Florida State University

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Big-City Police *by Robert M. Fogelson. Cambridge, Mass., Harvard University Press, 1977. 374 pp. $17.95.*

This study provides a prudent and comprehensive history of twentieth-century urban policing. Based on a thoughtful reading of primary and secondary sources, Robert Fogelson, an MIT historian best known for his history of Los Angeles, has written the volume most likely to become the standard work on its subject. In clear and direct prose the author traces the development of big-city police forces from their position as adjuncts of party machines at the turn of the century through two distinctive periods of police reform to the uncertainties of the present. The author's interpretations conform with the data, and the conclusions are sound. Overall, Fogelson argues, reformers have had mixed success in achieving their institutional goals, but have been more able to win ideological triumphs of major proportion. This record has made it difficult for blacks and other minorities to secure the place white, ethnic immigrants had on urban police forces. This has also seriously exacerbated police-community relations. The price of reform has been high.

In making this broad argument, the author illuminates not only the history of big-city police, but also the general trajectory of municipal reform. The police forces that were the targets of municipal reformers in the early twentieth century were quite different from their Western European counterparts. U.S. police forces were committed to local, rather than national, control; a civilian, rather than a military, orientation; and a style of policing that was responsive to the various mores of different classes and ethnic groups. Further, American police were set apart by their relationships with the political machine, especially with its decentralized ward organizations. Thus, a mutually beneficial exchange relationship developed between ward bosses and neighborhood police.

Reformers in the first three decades of this century were offended by this relationship and sought to alter it fundamentally. Largely drawn from the upper and middle classes, reformers of the Progressive movement attacked conventional police practices as an aspect of a larger campaign directed against local political organizations. This assault, Fogelson observes, was based on three closely related assumptions about society: "First, they believed that social mobility was an economic, private, and individual process, as opposed to a political, public, and collective one, and that success was a result of industry, frugality, integrity, and occasional good luck. Second, they held that political legitimacy was a function of the public interest, the common objectives of the entire community, and not of the parochial interests of particular neighborhoods, ethnic groups, and social classes. And third, they thought that American morality was based on a commitment to abstinence and respectability, and abhorrence of self-indulgence and deviance, and a willingness to employ the criminal sanction to distinguish the one from the other" (p. 47). The reformers rejected local control, political decentralization, cultural pluralism, and an emphasis on collective social mobility.

By 1930 the activities of these reformers had only marginally changed the character of local police departments. Ward leaders still dominated personnel decisions; most policemen were drawn from white, ethnic immigrant groups; and differential policing remained the rule. Yet this first wave of reform had two major enduring affects. First,

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it facilitated the development of an occupational identity for policemen, which from the start contained a serious level of paranoia. Not until the early twentieth century did policemen "perceive themselves as policemen, as opposed to laborers, clerks, and railway conductors temporarily employed as policemen. And by virtue of the reform crusade a paranoid style emerged from the very start as an essential component of this occupational identity" (p. 116). Second, by the early 1930s the reformers had won a major ideological victory: Their denial "that public service was a means of social mobility, that local control was a source of political legitimacy, and that immigrant-lifestyles were an expression of American culture" (p. 136) came to be widely diffused in the society.

This inheritance of the first wave of reform paved the way for the second wave, which dates roughly from the middle 1930s to the late 1960s. This era of reform differed from the first in a number of respects. Its leaders, generally senior members of big-city police forces, adopted a professional model of policing rather than a military model. They shifted the stress of their complaints from political interference to the internal organization of police forces themselves. Above all, they sought to achieve for the police the kind of professional autonomy achieved by doctors, lawyers, teachers, and other professionals. In this way, they suggested, police forces would be upgraded, and the traditional aims of the first generation of reformers could be achieved: efficient service at a low cost; insulation from political interference; and, most importantly, a single standard of law enforcement.

According to Fogelson, these reformers had important successes. They reorganized their departments along functional lines, established special units, weeded out incompetent policemen, and raised the pay and working conditions of their fellow officers. Although some of the worst abuses of the past were eliminated, urban crime proved impervious to these organizational transformations, which in fact sometimes made the situation worse. The creation of functional squads, for example, often shifted opportunities for corruption away from the precinct to the domain of the squad and "from the ward politicians to the policy bankers, the narcotics dealers, and other disreputable types" (p. 225). Thus the police in the 1960s were not much more efficient than the police in the 1930s. Most of their time, perhaps 80 percent, was spent on noncriminal matters.

The marginal gains had to be balanced against the cost of reform. Fogelson stresses that occupational paranoia grew much worse. The emphasis on professionalization helped create closed communities of police officers who saw themselves as apart from those they policed. Further, the professionalization of police standards, as well as the elimination of most residency requirements for police, made it exceedingly difficult for black, Mexican, and Puerto Rican newcomers to the city to compete effectively for police jobs. The reformers' doctrine of preventive policing often led to the harassment of people on the street and therefore to an exacerbation of police-community tensions.

None of these perspectives about the impact of reform is new of course. Indeed Fogelson's arguments echo the conventional wisdom of most of the urban social movements of the 1960s. But the strength of this volume is that it moves significantly beyond rhetoric to careful documentation; and it provides an accessible, readable, and well-researched reemphasis on the essential correctness of many of these themes. The book

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should be widely read and used; I hope a reasonably priced paperback edition will be forthcoming shortly.

IRA KATZNELSON
University of Chicago

The Cop Who Would Be King: The Honorable Frank Rizzo by Joseph R. Daughen and Peter Binzen. Boston, Little, Brown and Company, 1977. 344 pp. $10.00.

America has produced a rich variety of rogues in political life but it is doubtful if any of them has been as overtly and persistently outrageous and ludicrous as Frank Rizzo during his reign as mayor of Philadelphia. What politician but Rizzo would unabashedly challenge a rival, who accused him of dishonesty, to confront, in public, a lie-detector test? To compound the absurdity of the event, shortly before taking the test, Rizzo expressed his confidence in the polygraph and declared, "If this machine says the man lied, he lied" (pp. 237–238). Rizzo managed to state his name, and answer other introductory questions correctly; on all other questions he lied.

Other outrageous acts, which resulted in more serious consequences, included his organizing a special police squad to watch and harass his political enemies; encouraging his labor union cronies to shut down the *Philadelphia Inquirer*, with whom the mayor was quarreling at the time, by mob action and siege tactics; disrupting the school system and nearly bankrupting the city; overloading the public rolls with political hacks; and, most sinister of all, exacerbating racial tensions by championing the "cause" of Italians, Irish, Poles, and lower-middle-class Jews.

Joseph R. Daughen and Peter Binzen, senior reporters with the *Philadelphia Bulletin*, describe in some detail these and similar acts in a lively journalistic style. The book is clearly a polemic, hell-bent to show Rizzo as a disaster in every respect. "His leadership as mayor," the authors state, "was non-existent. Not a single program, not a single goal, not a single display of vision marked his tenure. He proved to be a do-nothing mayor with a big mouth who spent his time trying to destroy his enemies while building his own power base, all at taxpayers' expense" (p. 328). Although the study tends to be superficial and too narrowly focused, excluding the underlying social and economic forces at work in the city, it is clearly worth reading by political scientists.

As the established hero of white ethnic "row house" Philadelphians, Rizzo knows that he is more or less immune from attack by his liberal and black critics. The more they strike out against his outrageous acts, the more it confirms in the eyes of the whites that their man is indeed the tough cop who is prepared to fight to the last for "law and order." The business community—perhaps despite rather than because of his buffoonery—also appeared to be enamored of a mayor who was able to keep Philadelphia free from riots. They expressed their appreciation by contributing heavily to his campaign fund. By welding this unusual alliance of the white working class and the business community, Rizzo was able to ignore the needs and demands of the black community, which com-

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prises about 40 percent of the city's population, and still win elections. (He will run for an unprecedented third term if his supporters succeed in appealing the city charter's provision limiting a mayor to two terms.) Thus on a strictly "law and order" stance, the mayor of the fourth largest city in the nation has succeeded in using race as means to achieve and sustain power. Another victory of this kind in the next election could well signal the downfall of an American city.

PETER BACHRACH
Temple University

Metropolitan Financing and Growth Management Policies: Principles and Practice *edited by George F. Break. Madison, Wisc., The University of Wisconsin Press, 1978. 329 pp. $15.00.*

The ten chapters in this volume constitute the proceedings of the 1974 Conference of the Committee on Taxation, Resources, and Economic Development (TRED). The conference was unabashedly defined by "the working research of the participants" rather than by a "concentration upon some predetermined issue" (pp. 3–4). The result—which will not surprise readers of earlier TRED volumes—is a collection of empirical and theoretical papers that examine economic aspects of property taxation, land value taxation, and land use.

The papers are arranged in three loose groups: growth management, metropolitan finance, and property tax reform. In the first section, William H. Oakland surveys the effects of local taxes on intraurban industrial location; Richard W. Lindholm describes the legal and administrative features of Oregon's land use control program; and George E. Peterson appraises the impact of federal taxation on the conversion of land at the urban fringe. The metropolitan finance chapters include one theoretical paper by Peter Mieszkowski that evaluates mechanisms for urban land use regulation and another by Donald C. Shoup that estimates the effects of property taxes on the capital intensity of urban development. This section also presents Gail C.A. Cook's analysis of the consequences of the creation of metropolitan Toronto for the costs of public services and Roy W. Bahl's survey of the use of property taxation to guide urban development in nonindustrial nations. In the final section of the volume, Larry D. Schroeder and David L. Sjoquist explore the distributional impact of homestead exemption proposals; Daniel M. Holland and Oliver Oldman estimate the implications of full-value assessment in Boston; and Arthur P. Becker and Hans R. Isakson simulate the consequences for Milwaukee of a shift in the funding of certain public services from a local property tax to a statewide income tax. The book concludes with a summary of conference discussions on transitional issues in land taxation and the fiscal dimensions of metropolitan consolidation.

Some topics in this collection will be of greater interest to political scientists than others. Among those is Lindholm's description of the Oregon program adopted in 1973 to use administrative procedures and financial incentives to preserve the nonintensive use of undeveloped areas, but regrettably this chapter is perhaps the least satisfactory contribution in the volume. It summarizes the legal provisions of the program that mandate

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goal setting, citizen participation, comprehensive plans, and a greater state role in land use decisions, and it records a shift during the first years of the program's operation from a concern for lakes, streams, and outdoor recreation to an emphasis on the expansion of employment opportunities. However, Lindholm does not bring the features of the program or the events into sufficiently precise focus to permit systematic examination. His discussion would have been enriched by a more explicit grounding in the literature that appraises analogous procedures in other states and in other programs. His conclusion that the Oregon program is "at least partially successful" (p. 48) may be accurate, but it is uncomfortably detached from the information presented in the chapter.

Cook's analysis of the impact of governmental reorganization in metropolitan Toronto is substantially more rewarding. This study examines the hypothesis that the centralization of financing a public service that had previously been characterized by significant interlocal disparities in expenditure levels will increase total expenditures for that function. The equalizing-up hypothesis was solidly confirmed for education and somewhat less so for fire. Police expenditures, which were not affected by the governmental reorganization, showed no significant changes during the research period. The elimination of tax differentials was not consistently translated into changes in residential property values. Becker and Isakson also contribute a revealing analysis of the consequences of a proposal widely advocated in governmental circles—the shifting of the funding of urban services from the local property tax to a statewide income tax. Using data from Milwaukee, they conclude that a change in financing elementary and secondary education from the local property tax to Wisconsin's progressive income tax would lower the tax burden for the richest and poorest households in the city and moderately increase the tax burden for everyone else. The effects of shifting the funding of other services are by no means obvious or unambiguous.

The chapters in this volume are disparate ones, but together they demonstrate again the need for students of both public finance and political science to assimilate the work of the other more thoroughly.

RICHARD LEHNE
Rutgers University

Protest, Politics, and Prosperity: Black Americans and White Institutions, 1940–75 *by Dorothy K. Newman, et al. New York, Pantheon Books, 1978. 348 pp. Cloth, $15.00; paper, $4.95.*

This volume was supported by a grant from the Carnegie Foundation and was written by a team of six social scientists. The preface to the volume states its theme as an analysis of "how black Americans make progress or are thwarted in achieving it" (p. v). The bulk of the text consists of an analysis of racial discrimination in American society, the forces that produced the civil rights movement, the legislation passed, the quality of this legislation and efforts to enforce and implement it, and the extent of progress in overcoming racism. Six policy areas are examined: employment, educational training for employ-

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ment, job discrimination by the federal government, housing, medical care, and economic security.

The individual policy chapters are well done. The nature of discrimination in each area is graphically reviewed. The events leading to protest and legislation to alleviate these grievances are examined, and an assessment of progress provided. The analyses are generally solid, hardnosed, and uncompromising. The inevitable conclusion for each policy area is that while some progress has been made, often important progress, black Americans are still far from equal with white Americans; discrimination still exists; and the quality of efforts to overcome continuing discrimination is at least inadequate, sometimes grossly so.

The authors emphasize that protest was, and continues to be, essential to the alleviation of racial inequality. "The cutting edge of black Americans' progress . . . has been protest. Our findings are unequivocal that vigilant, aggressive protest is necessary at all times—good and bad—to maintain gains, to prevent losses, and to make progress" (p. 285). While this argument is repeated often, it is not well developed. If after twenty years of contemporary struggle black families still earn only 59 percent of white median family income (1976); if metropolitan schools are still segregated; if 81 percent (or more) of all Americans still live in segregated neighborhoods; if blacks still hold less than 1 percent of all elected offices; what kind of protest would be necessary actually to close the gap between whites and blacks over the next twenty years? Is massive mobilization necessary? Must cities burn? As limited as this volume shows progress to have been, the inescapable conclusion is that massive protest and perhaps disorder would be required. The authors, however, never develop this point. They seem to shy away from the implications of their own research and conclusions.

Additionally, while the authors are quite critical of black unemployment rates and economic decisions that stress controlling inflation rather than promoting full employment, they do not raise questions about whether our current economic system can ever provide stable full employment along with low inflation. Subemployment is not discussed, and the whole history of economic instability in the form of recessions, depressions, panics, inflation, and persistent high unemployment is given little attention. The authors seem to know that some exceptional reforms would be necessary to create an economic system that could serve all Americans but are disinclined to probe and speculate about this tough and controversial problem.

Also given short shrift is the nature of the system of political power in America. Thwarted civil rights goals are mostly blamed on a handful of racist members of Congress and unsympathetic presidents (Eisenhower, Nixon, and Ford). But the problem is certainly more complicated than this. Why do blacks have so little influence in the American political system even during administrations in which they provide the margin of victory (for example, Truman, Kennedy, and Carter)? Is it because the system is rigged against change or biased in favor of upper-income groups who prosper from the status quo? The authors seem to suggest so, but again they seem reluctant to flesh out their conclusions.

The strength of this volume lies in its review of progress in various policy areas. If the authors have any insights into the systemic factors that continue to retard racial

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equality in America, or what black Americans and their allies will have to do to overcome these barriers, they do not develop them here.

HARRELL R. RODGERS, JR.
University of Houston

Black Labor and the American Legal System, vol. 1, Race, Work, and the Law *by Herbert Hill. Washington, D.C., Bureau of National Affairs, 1977. 455 pp. $17.50.*

Parts of this book are stimulating, interesting, and worthwhile. Unfortunately, although the author deals intelligently with his subject, the book lacks cohesiveness and needs further editing. The writing is uneven, and the overall organization is somewhat shakey.

Race, Work, and the Law is the first part of a projected two-volume study dealing with the black worker and the American legal system. Volume I is concerned primarily "with the events that preceded and affected the enactment of Title VII" of the Civil Rights Act of 1964 that prohibited employment discrimination, "including the consequences for nonwhite workers of the National Labor Relations Act . . . and government efforts to eliminate job discrimination through administrative agencies, with special attention to the work of the original Fair Employment Practice Committee during the 1940s" (p. 2).

The overly short and superficial introduction outlining "the historical sources of resistance to change" in black employment in the United States neglects most recent scholarship (for example, Philip Foner's *Organized Labor and the Black Worker in America*) and is heavily dependent on works written in the mid-1960s and earlier. After this somewhat limited opening survey there are chapters that deal informatively with judicial attacks on the "inherently discriminatory aspects of employment systems" (p. 62) in the ten years since Title VII was enacted; "the contemporary application of the Civil Rights Act of 1866" (p. 63); and the "history of the National Labor Relations Board in regard to racial discrimination" (p. 93) which Hill angrily (and perhaps somewhat unfairly) characterizes as a "general unwillingness to use" the NLRB's "powers against discriminatory employment practices" by both management and organized labor.

The second half of this book deals in general with federal executive orders that were designed to prohibit employment discrimination during World War II. The emphasis is on the activities of the Fair Employment Practice Committee (FEPC). This portion of the book is no less polemical than the preceding chapters, but its extensive (often detailed) analysis of job discrimination in particular industries such as shipbuilding, trucking, transit, and railroads gives an informed and useful review of the problems faced by black workers and their allies of all races in combatting job discrimination. Hill has made fine use of interviews in this section.

Although these chapters are mainly concerned with FEPC treatment of the problem of job discrimination during World War II, brief summaries of events since the war are given at the end of some chapters. Despite all that has been done to achieve economic

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justice for all, however, much still remains to be undertaken in ending Jim-Crow job practices. "We've come a long way, but not far enough" is Hill's message to the reader.

Much of this book reads like a brief, and given Hill's past associations this is perfectly understandable. A veteran civil rights activist, and for many years National Labor Director of the National Association for the Advancement of Colored People, the hard-working, dedicated Hill was intimately involved with the cases and events that he describes in this book. Although now a professor at the University of Wisconsin at Madison, his writing clearly demonstrates that he has lost none of his zeal or fervor. He remains an ardent advocate of economic justice.

Unfortunately, an impassioned plea, no matter how well buttressed by scholarly apparatus, rarely makes for an outstanding study. Hill's book, though quite valuable, is no exception to that rule and ultimately falls short of being the comprehensive, interpretative history it claims to be.

DANIEL J. LEAB
Seton Hall University

Cancer Crusade: The Story of the National Cancer Act of 1971 *by Richard A. Rettig. Princeton, Princeton University Press, 1977. 382 pp. $15.00.*

All who are informed about the subject seem to agree that the passage by the U.S. Congress of the National Cancer Act of 1971 was a political event of intrinsic significance. The author of this volume, by his own declaration, is attempting to reach three broad audiences: the biochemical scientific community and others who carry out U.S. policy toward cancer; the general public; and political and social scientists. Individuals in all three audiences will find here a clear narrative which describes how a small but powerful elite composed of private citizens mobilized sufficient political resources to secure passage of this legislation. The group realized its objective, though it was opposed by the National Institute of Health and by most of the biochemical scientific community.

The study will be of most interest to students of politics because of the author's attempt to cast the discussion in the context of what some political scientists call "agenda setting." He argues that the agenda for cancer research—and hence the legislation being discussed—was set and nurtured by the philanthropist Mary Lasker and her associates. The source of the initiative leading to the cancer legislation was not the Congress, the executive branch, the relevant professional community of scientists, or an organized interest group, but was instead "a unique constellation of private citizens grouped around Mary Lasker" (p. 282).

Probably few observers who have followed the National Cancer Act closely will find little startling new information in the book. But those scholars seeking suggestive agenda-setting concepts will likely find the work of value. The author himself refrains from predicting how cancer research will affect public agenda setting in the immediate years ahead in the broad biomedical area. But one gains the impression that he feels the future agenda will include a greater emphasis upon a cancer prevention strategy as opposed to the overwhelming energies and dollars now applied toward cures. The author notes, accurately,

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that environmentalists and others have claimed that the cancer effort is misdirected toward cures rather than a prevention strategy that seeks "the withdrawal of chemical carcinogens from food, the workplace, the environment, and consumer products" (p. 318).

To this reader the author's major contribution is his brief speculation upon the possibility of greater research on the desirability of withdrawal of such carcinogens. It could be argued that his stated objective does not include more than cursory attention to this matter. But it is disappointing to the reviewer that his comments in this regard were merely tantalizing. His most dramatic assertion is simply that "the pressure for action has emerged as much from publicity and public opinion about egregious instances of chemically-induced cancer as from normal decision-making channels" (p. 306).

Perhaps the political bottom line with respect to agenda setting for future cancer research is recognition that a more intense examination by the federal government of the American chemistry industry could be involved in the next cancer agenda-setting exercise. Supporting regulation of the massive chemical industry is a role far afield from the traditional research orientation of the National Cancer Institute (NCI) and other federal agencies. One can hardly imagine the level of emotion which would accompany the conflict between the federal government and the chemical industry if such a regulatory effort were to be mounted.

Yèt one cannot complete a reading of this book without wondering if the author does not really believe that the greatest potential cancer crusade lies in dealing with the chemical nature of our industrial society. This potential, the author maintains (p. 323), was not appreciated at the passage of the National Cancer Act.

BOYD R. KEENAN
University of Illinois at Chicago Circle

Comprehensive Income Taxation *edited by Joseph A. Pechman. Washington, D.C., The Brookings Institution, 1977. 311 pp. Cloth, $11.95; paper, $4.95.*

Eight papers prepared for a conference on comprehensive income taxation at The Brookings Institution late in 1976, together with an appendix containing estimates of the yield of a comprehensive income tax, appear in this volume. They give a clear and complete survey of the main conceptual and administrative problems raised by proposals to broaden the base of the U.S. income tax with a compensating reduction of rates.

In the first paper Richard Goode reviews the historical development of the "Schanz-Haig-Simons" definition of income as the sum of consumption and increase in wealth over a period of time, and points out the main respects in which current definitions of taxable income diverge from it. John F. Due discusses what personal deductions currently allowed are compatible with the S-H-S concept, suggests practical ways of bringing the present law into conformity with the S-H-S definition, and calculates the changes in tax revenue which might be expected from the proposed alterations. Emil M. Sunley, Jr. wrestles with the knotty problem of taxing employee benefits, especially rights to pensions vested in the present but payable as cash only in the future, and discusses the tax treatment of government transfers. James M. Wetzler proposes the full taxation of capital

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gains as they accrue, or the nearest administratively feasible approximation of such a policy, and discusses the consequences of this reform on revenue, income distribution, and horizontal equity. William F. Hellmuth studies the variety of preferences accorded homeowners under the present law and the feasibility and desirability of eliminating them. Michael J. McIntyre and Oliver Oldman take on the controversial question of the appropriate tax treatment of family units, arguing that the S-H-S definition of income by implication suggests taxing each individual on her or his own consumption and increase in wealth. E. Cary Brown and Jeremy I. Bulow contribute a discussion of taxation of business income centered on the problem of divergence of the economist's concept of accrued income with perfect markets from administratively determinable measures of realized income. Emil M. Sunley, Jr. appears again to summarize the conference discussion. Joseph J. Minarik's appendix estimates the yield of tax reform packages combining the various features studied separately in the other papers, which shows the substantial reduction in tax rates, and thus presumably in costs incurred and distortions created by taxpayers' efforts to avoid taxes, made possible by these reforms. Each paper is accompanied by comments written by other conference participants.

The papers are at their best, in general, in discussing the practical difficulties and consequences of the reforms at issue. A particularly useful feature of the book is the wealth of calculations of the fiscal consequences of the various reforms, and estimates of their effect on income distribution and horizontal equity. A participant in the legislative process who is putting together a package of tax reforms will find this material an invaluable starting point in costing out and estimating the impact on particular classes of taxpayers of various combinations of proposed reforms.

At the theoretical and political level the book is weak. There is no careful and systematic discussion of the numerous conceptual problems involved in any definition of income (particularly the extent to which workers' consumption is in fact a cost of reproducing their power to work), nor of the real distinctions between different kinds of income (especially income from property and from work), nor of the rationale behind using income as the base of the most important national tax. It is not possible to resolve some of the problems raised in the papers (such as the deductibility of education expenses, the appropriate treatment of capital gains to achieve horizontal equity, or the appropriate taxation of business income) without taking a position on these basic philosophical and political questions. The political premise underlying the book is the somewhat naive idea of trading special tax preferences for uniformly lower rates and a simpler tax system (though as the discussions show, making the tax system more uniform and comprehensive often makes it more complex). It might have been helpful if some experts on the legislative process and the politics of tax reform had addressed the political realities which shape legislation in this area as part of the conference on which the book is based.

There is considerable repetition in this volume. Some of this is the result of the publication of a series of separate papers, but some could have been avoided by editorial decision. The first and eighth essays cover very similar ground, and the appendix duplicates, from a slightly different perspective, discussions in several papers. The paper on tax treatment of the family seems only tenuously connected to the overall theme of the book, which is the definition of taxable income, not of the taxpaying unit. The essay on business income taxation also seems somewhat out of place, since its theoretical discussion is

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considerably more elaborate than the rest of the papers, while its discussion of practical reform problems overlaps the earlier paper on treatment of capital gains.

Comprehensive Income Taxation is a coherent and careful attempt on the part of economists to inform and influence from their own expert perspective the continuing national debate over tax reform.

DUNCAN K. FOLEY
Barnard College, Columbia University

Welfare: The Political Economy of Welfare Reform in the United States *by Martin Anderson. Stanford, Hoover Institution Press, 1978. 251 pp. $10.00.*

Martin Anderson begins by stating that "since the middle of the 1960s, welfare reform has been a major issue on the political agenda of the United States. . . . But all attempts to radically reform the welfare system have failed. . . . the question is, why?" (p. 3). This book is Anderson's attempt to answer this question and to offer his recommendations for a viable welfare program. Neither attempt is successful.

A major problem with the book is the author's failure to clarify who his audience is. If Anderson is writing for scholars and policy analysts knowledgeable about the field, the book is too elementary and written in far too patronizing a manner. If he is writing for the general public, there is too much detail and repetition. An article would have sufficed.

Anderson makes two main points, both of which have been made elsewhere by many others. First, any assessment of the impact of income maintenance programs on the incidence of poverty in the United States must include in-kind, as well as cash, transfers, and must take account of spillover effects of cash transfers, such as household splitting (for example, creating two low-income households out of one moderate-income household). His conclusion—that there is less absolute poverty in the United States than is commonly thought—has been reported with some regularity over the past few years. However, Anderson ignores both the complexities of the definition of poverty and the concomitant difficulties in measuring the effects of cash and in-kind transfers on individual and family income. (For example, how do you assess the value of medicaid to a family that has had no medical care that year?) But that is a separate issue.

Anderson's second point is that all efforts at comprehensive welfare reform are doomed to failure (and this includes the Carter proposal) because it is impossible to design a program that will simultaneously accomplish three primary goals: adequate benefit levels; adequate work incentives; and reasonable costs. Here, Anderson is beating a dead horse. No one seriously expects the Carter welfare reform proposal to be passed, and most professionals working in the income maintenance field expect at best some sort of incremental reforms. Unfortunately, Anderson also does not even begin to explore whether there might be politically viable ways to achieve these goals separately, through alternative approaches.

The author concludes his study by lambasting the Carter proposal, and then offering his own proposal for something akin to the traditional Poor Law: a highly decentralized

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program for the needy with a rigorous work test. Anderson ignores the inherent goal conflicts in his own proposal between, for example, adequate benefit levels and a highly decentralized program with great local variations. Of even greater importance, he ignores the most critical questions for a cash assistance program: Who are the needy? What criteria determine employability? How can an employment test be fairly enforced? What is to be done about those who are employed full time and thus ineligible for these programs, yet have incomes below the poverty level? These questions still require attention.

SHEILA B. KAMERMAN
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Columbia University

Unemployment in History: Economic Thought and Public Policy *by John A. Garraty. New York, Harper & Row, 1978. 273 pp. $15.00.*

Unemployment as a term apparently does not antedate its use by the Massachusetts Bureau of the Statistics of Labor in 1887. Unemployment as a condition may well parallel the rise of early civilizations, but as John Garraty suggests, its conception as a state differing from morally condemned idleness is relatively recent and still subject to a variety of definitions. What Garraty has tried to do in this book is not to present a comprehensive history of unemployment through the ages, but, rather, to examine how economists have viewed it; how their ideas have affected government policies; and how, in turn, public programs and actual conditions have influenced further economic thought.

After a brief survey of how little is known of the classical world, the author rapidly transits the Middle Ages to discuss the changes wrought between the thirteenth and sixteenth centuries, which produced large numbers of poor and unemployed. The ruling classes met this perceived threat to their health and safety in various ways. For the old, sick, and incompetent—the unemployables—they established a welfare system with support drawn from general funds. For the wandering surplus laborer, however, there was neither sympathy nor charity. The medieval assumption that work was available for all was not challenged, and the unemployed often faced hard, forced labor both as a punishment and as a warning.

In the emerging nation-states of Europe, work meant strength, and joblessness became the equivalent of treason. John Locke could suggest then, in 1697, that the idle be forced into naval service or workhouses and their children over the age of three be placed in work schools.

The early scientific economists tended to ignore unemployment. Adam Smith denied that man was naturally lazy, and he attacked low wages as harmful to all of society. Still, he along with Malthus and Ricardo joined their predecessors in viewing involuntary joblessness as unimaginable—work aplenty was available for those who sought it.

The cycle of industrial depressions in the nineteenth century brought a dawning realization that they were accompanied by involuntary idleness. The theories of the classical economists were at their most influential, however, and both the poor and the unem-

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ployed were held responsible for their own plight, especially in an economy in which growing prosperity brought increasing complacency for the already comfortable.

Karl Marx saw the unemployed as an absolute necessity for capitalism. They were its industrial reserve army, and only the elimination of capitalism would solve the problem. Other economists adopted his concept but not his solution.

By the 1890s unemployment had received its name and the recognition of its existence in capitalist society. From that point onward, the question for nations became how best to cope with the jobless condition. One school would fight to reduce or even prevent it; another would simply try to provide support for the unfortunate. Increasingly in this century the United States and the countries of Europe have taken responsibility for the unemployed. Britain's unemployment insurance system, for example, expanded rapidly after World War I.

The United States lagged in government programs during the 1920s because scientific management in business was supposed to eliminate joblessness. The Great Depression mortally wounded this faith and brought unprecedented unemployment. Like the plague, it struck some families without seeming reason while bypassing others.

Classical economists blamed government, business, and unions for stifling free enterprise. Their answer to the problem of unemployment was further deflation and lowered wages. This was politically impossible. Politicians struggled toward programs which crudely paralleled John Maynard Keynes's ideas in *The General Theory of Employment, Interest, and Money*, published in 1935.

Keynesian economics, finally accepted by most western governments after World War II, gave promise of a golden age of full employment. Alas, however, as Garraty concludes, Keynesian economics perhaps no longer works in a world which seems to combine both inflation and a high rate of unemployment. He offers a series of thoughtful questions but pretends to no answers.

Like all books which break new ground, Garraty's work will have severe critics. Many historians will probably see it as too theoretical. Economists may well find it simplistic. Lord only knows about the sociologists. It is easy to carp, but the author has drawn on an amazing number of sources and has done an excellent job. The work should provoke further research and discussion, and that is all to the good.

LOWELL K. DYSON
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The Making of Political Women: A Study of Socialization and Role Conflict *by Rita Mae Kelly and Mary Boutilier. Chicago, Nelson-Hall, 1978. 368 pp. $16.95.*

The use of the plural—women—in the title of this interesting book clews us in at once to the fact that it is an empirical study of actual women—thirty-six, of whom twelve are wives, ten achievers, and fourteen revolutionaries—rather than of an abstraction, "woman." It "is concerned with how real political women and political wives developed the traits and behavior patterns they did, not how either their psychology and personalities varied or how women in general vary from each other or from men" (p. 24). It is

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based on the work of Maslow, Rogers, Angyal, and Goldstein on needs, and of Renshon on political efficacy (p. 3).

The book's theory posits four stages that must be passed through sequentially to become an achieving political woman: "First the child must develop an activist sex-role ideology; second, she must gain personal control over her life-space, gaining needed competencies and abilities to maintain control as the life-space expands; third, politics must become personally salient to her; and fourth, at various points of the life cycle her efforts must have been sufficiently rewarded and her experiences sufficiently successful to encourage her to continue" (p. 312).

The women fell into six types: (1) private, traditional women; (2) terrorists; (3) public women/political wives (of executives); (4) elected women whose careers had been mediated by men; (5) women elected on their own; and (6) revolutionaries. The four-stage model applied to them found that they fell along a gradient in terms of sex-role ideology, control and extent of life-space, exposure to political concerns, and overcoming early poor political socialization. Surprisingly, the terrorists resembled the traditional women except that they had been oppressed in the traditional family setting and the revolutionaries resembled the elected women except that they opposed rather than supported the status quo. The model of the elected political woman is clearly the most admired by the authors. She had socialization for independence, did not accept the "presumed restraints of being a wife and/or mother" or recognize limitations on what she could do (p. 324). Attempts to defeat her just because she was a woman met with belligerent response. She expected no insurmountable restraints and refused to accept the legitimacy of any.

The authors believe their study has implications for socialization in general and political socialization in particular, for social movements, for specific types of political behavior, and for the study of female behavior. The lessons they draw are that "politics is not inherently a 'masculine' activity" (p. 331); that participation by women will not have a marked effect on political life or policy (p. 331); that more research is needed on the success of political women rather than on explanations of past hindrances (p. 332). They believe our culture can and should stimulate political competency in women (p. 334).

This is a valuable book. It adds to the growing body of knowledge about the world of women which has been so neglected in the past. A rather unexpected conclusion and recommendation to feminists is, however, surprising. The authors seem to accept the allegation that the women's liberation movement in the 1970s is responsible for the upsurge of single-parent families and a doubling of the divorce rate (p. 336). Since political achievers come from intact families based on love, esteem, and respect, as well as equality, the feminist movement is counterproductive, destroying the very matrix in which political achievers are molded. "Not only all women but also all men ought to be concerned about correcting those features of society and its ideologies that give rise to the current sharp increase in negative socialization environments" (p. 336). Do they imply that the women's liberation movement is not concerned about correcting them?

JESSIE BERNARD
Washington, D. C.

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The Pay Board's Progress: Wage Controls in Phase II *by Arnold P. Weber and Daniel J. B. Mitchell. Washington, The Brookings Institution, 1978. 454 pp. Cloth, $12.95; paper, $5.95.*

As popular demand for control of inflation persists, the Brookings Institution's series of studies of past U.S. experiences with incomes policies, to which this book belongs, will be an important source of lessons. This volume deals with the program of wage controls, lasting from November 1971 to January 1973, that succeeded the three-month wage-price freeze imposed by President Nixon in August 1971. The authors were important participants in Phase II: Weber was a public member of the Pay Board throughout its existence; Mitchell was its chief economist. They try to do three things: provide a detailed chronological account of the activities of the Pay Board, how the organization was set up, and how its policies and methods of operation evolved; analyze the Pay Board's impact not only on wage growth but also on the workings of the labor market and collective bargaining; and communicate their own judgments as participant-observers on the episode and its lessons for the future.

This is a book for the specialist. The authors assume the reader has a newspaper knowledge of Nixon's programs of wage and price controls and a good memory. They go into more detail about the Pay Board's activities than most readers will find interesting, but in doing so they provide valuable source material for historians. The book would be more useful if it began with a summary of the chronology and main features of the various phases, providing a setting for this detailed account of the Pay Board in Phase II.

As participant-chroniclers, Weber and Mitchell belong to a venerable tradition of historical writing that has inherent strengths and weaknesses. As insiders they can tell us things no one else can, giving their account depth and authenticity. Their opinions, grounded in firsthand experience, are bound to carry weight. However, the authors inevitably arouse the suspicion that their book does not do justice to other viewpoints. Weber and Mitchell try hard to be evenhanded, but they make no secret of their disagreement with the AFL-CIO's view of the Pay Board's record. Their effort to be dispassionate weakens the book. Too often the authors refrain from expressing their own opinions where we would like to know them. The reader still wants to hear directly from the other side before forming a judgment on labor's role in the Pay Board, and other controversial issues.

Two of the authors' conclusions will be of special interest. First, from their experience of the Pay Board's functioning both as a tripartite body (with labor, management, and the public equally represented) and as an all-public body (after four of the five labor representatives resigned in March 1972), Weber and Mitchell conclude that a tripartite structure has more disadvantages than advantages and that an all-public body can successfully control wages.

Second, from their analyses of internal data on the disposition of cases and of external data on wage movements and bargaining patterns in various sectors of the American economy, the authors find that the Pay Board reduced the rate of wage growth below what it would otherwise have been. It did so without suppressing an inflation until after controls were lifted and without destroying established patterns of collective bargain-

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ing. The evidence they amass should correct the view that the Nixon experiment with direct wage controls was a disaster.

The real question is not whether controls can work, but under what circumstances, and for how long, can they work? From this account it seems clear why the Pay Board worked as well as it did. During its existence the labor market was slack, and upward pressure on wages was relatively weak. The Pay Board set permissible increases at a high enough level and interpreted its own rules with sufficient flexibility that few firms or unions badly wanted more than they got. This book should therefore not be taken as evidence for the workability of wage controls in all situations, but only for their feasibility under certain conditions.

CORDELIA REIMERS
Princeton University

British Foreign Policy under Sir Edward Grey *edited by F.H. Hinsley. Cambridge, England, Cambridge University Press, 1977. 702 pp. $57.50.*

Within its self-imposed limits, which are more severe for political scientists than for historians, this is a fine and informative volume. The limits are several. As with most sets of essays, there are repetitions and lacunae. Moreover, the concentration on British foreign policy, although producing an admirable focus, neglects the international environment in which Great Britain operated. Yet to understand, and still more to judge, the appropriateness of British foreign policy, one must delve more deeply into both the ideas and general milieu that were shared among nations and the policies of the countries England dealt with. Although Jonathan Steinberg's excellent chapter on the German background to Anglo-German relations starts to fill this gap, it is not sufficient. A more complete picture—which of course can be provided by other books—requires more analysis of what other countries sought, how they saw Great Britain, and how they responded to her policies. On these matters, of course, hinge the crucial questions of whether Britain's policies were appropriate and efficacious, or whether an alternative approach, such as the obvious one of being more conciliatory toward Germany, could have avoided the First World War.

Although Sir Edward Grey figures prominently in all the essays (in addition to being named in the title), there is remarkably little said about his personality and general outlook. The foreign secretary remains a flat, one-dimensional figure. It is remarkable that not one of the thirty-one essays is devoted to Grey himself. Some of the authors mention Sir Edward's pessimism, especially in his later years, and one mentions the impact of the death of his wife, but no analysis is given. Since the premise of this book seems to be that Grey's tenure from 1905 to 1916 provided British foreign policy with some sort of unity, more should have been said about this coherence and about the way in which the foreign secretary left his stamp on policy. Furthermore, the omission of a summary chapter pulling together many of the themes of the individual essays and relating them either to Grey's beliefs and methods of operation or the broader historical issues of the period is unfortunate and makes the book less coherent than it might otherwise have

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been. Also, most of the essays are "old-style" diplomatic history—they concentrate on what the statesmen were saying to one another. The setting of domestic politics and shared attitudes is barely sketched in and little consideration is given to the possibility that the most important causes at work were those that are not adequately reflected in diplomatic conversations. As with much history, the essays are stronger on description than on explanation.

But within these limits, most of the individual essays are excellent. Organized both by geographical areas and periods of time, the important episodes in British foreign policy are discussed with great thoroughness and admirable clarity. Experts in the field will find new insights; neophytes will be well guided through many of the complexities of the period. Sweet's chapter on the Bosnian crisis, for example, is a model of lucidity.

Rather than attempt the impossible task of reviewing each essay, let me mention an important general theme of *British Foreign Policy under Sir Edward Grey* that may be new to most political scientists. Grey is seen neither as rabidly anti-German nor as the pawn of his permanent officials. Rather he is depicted as taking the middle ground between the officials' demands for wholehearted support of France and Russia and uncompromising firmness toward Germany, and the domestic political pressures from the wing of the Liberal party that sought an understanding with Germany. Furthermore, this was not merely political expediency on Grey's part. He was not entirely convinced of Germany's unremitting hostility; at several points Grey tried to move toward an understanding with her. Indeed, Michael Ekstein stresses that by 1914 British foreign policy was becoming much more conciliatory toward Germany. A second important theme concerns the growing friction between England and France and between England and Russia in the years before the war. Russia's increasing military strength not only alarmed Germany, but raised a steady stream of problems for Britain.

Students of international politics can learn an enormous amount from the pre-World War I era. In addition to having great intrinsic interest and importance, this collection of essays sheds light on innumerable questions of theoretical interest. Anyone concerned with topics such as alliances, bargaining, decision making, and the relationship between force and foreign policy can greatly enrich his understanding by consulting this book. Scholars working on the pre-World War I decade will not be able to skip this volume; students of international relations should not skip it either.

ROBERT L. JERVIS
University of California, Los Angeles

Technical Change and British Naval Policy, 1860–1939 *edited by Bryan Ranft. New York, Holmes & Meier, 1977. 178 pp. $19.00.*

This is an interesting collection of essays, but its value would have been greater had they been knit together, more sharply focused, and related to the general questions of innovation in large organizations. Although most political scientists probably would prefer a more systematic treatment, these essays provide the reader profit and pleasure for they deal with important subjects and tell their stories very well.

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Each of the seven essays is quite narrow, but many of the findings are likely to apply in other cases. Thus Philip Towle shows how participants on both sides of the bitter disputes about British policy used naval attachés' reports on the battles of the Russo-Japanese War. Geoffrey Till's analysis of the debates over the vulnerability of capital ships to air attack reminds us how difficult it is for an organization to examine critically a threat to its prime instrument and shows that even field tests yield highly ambiguous results. Hugh Lyon's analysis of the relationship between the Admiralty and the private ship yards gives perspective on current arguments about the "military industrial complex" and is particularly interesting in demonstrating the large role played by the companies.

The essays are strictly historical. They do not look at innovation or reaction to technical change in general. They do not compare the performance of the Royal Navy with that of other navies or with other branches of the British armed forces. The essays do not help us determine whether there is anything special about military organizations; nor do they draw on the analyses of other organizations that are confronted with new information and changing environments.

Although few explicit conclusions or generalizations are offered, the essays share a common perspective—sympathy with and criticism of the Royal Navy. Sympathy is derived from the authors' knowledge of the ambiguity of the information available to the decision makers, the complexity of their tasks, and the constraints on their resources. People and ideas that seem foolish in retrospect often had a lot to be said for themselves given the historical situation. Technology changed rapidly, and only a few intermittent wars allowed tests of competing theories. Furthermore, the dominant position of Great Britain meant that her ships had to be prepared for a wide range of disparate contingencies. She could not afford the luxury of highly specialized designs, but rather had to integrate many conflicting features into British ships (p. 58). And even when naval officers were on the right track, they were often blocked by insufficient funds. Innovation is not always possible, or wise when it is possible. Overall, the authors argue, the British navy was not as conservative as is often assumed; in many cases its caution was justified.

The authors also are critical of the quality of analysis displayed. The editor's observation about the British method of protecting merchant shipping against surface raiders is echoed in the other essays: the Royal Navy was preoccupied by what it considered to be "offensive" measures for hunting down the enemy and thus shunned convoys as "defensive" without careful examination of the issues and alternatives. "Without any naval staff," states Ranft, "there was no one to carry out the hard detailed thinking which was necessary" (p. 4). Alan Cowpe notes that many of the inadequacies in the navy's attempts to cope with torpedo attacks and the failure to come to grips with the question of how this weapon affected the traditional policy of blockade "stemmed essentially from a tendency at all levels not to look beyond the immediate issues and think problems through in any depth" (p. 36). Till points out that although aspects of air defense and vulnerability were closely scrutinized, "there was little continuing research into the battleship problem as a whole" (p. 119). Although the situation facing the Royal Navy was difficult, many of the wounds were self-inflicted by an organization that did not encourage

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careful, sustained analysis and too often relied on dogmatic reassertions of established traditions.

ROBERT L. JERVIS
University of California, Los Angeles

Political Integration and Disintegration in the British Isles *by Anthony H. Birch. London, George Allen & Unwin, 1978. 183 pp. Cloth, $16.25; paper, $7.50.*

This book presents the reactions of an English political scientist (now residing in Canada) to the recent growth of nationalism in Wales, Scotland, and Northern Ireland. Birch's study is worthy of attention for this reason alone, since this is a subject seldom taken seriously in English (as opposed to British) political science.

Birch feels that national identities in these regions have not been undercut by industrial development because "ethnic . . . loyalties are enduring features of social life" (p. 172). This enables him to find little merit in theories that suggest that the social base of minority nationalism lies in regional economic, political, and cultural deprivation. If ethnic identity is some kind of primordial sentiment, then the reasons the fortunes of the nationalist political parties have improved so rapidly must be sought in causes lying beyond each region's borders. The timing of the nationalist revival is due, instead, to three kinds of recent social changes. First are changes in the character of the international system. The size of a country has ceased to bear a close relationship to its military security. This has removed one of the main benefits to be derived from membership in a large state. Second, the development of supranational economic organizations has given the citizens of small states economic advantages that hitherto have been enjoyed only by citizens of sizeable states. Third, television's threat to the maintenance of minority cultures—far stronger than the threat posed by the other media—has provoked a hostile defensive reaction in the peripheries.

Since each of these institutional factors is likely to persist in the foreseeable future, movements like Welsh and Scottish nationalism can only continue to threaten the territorial integrity of multinational states like Britain. Thus Birch concludes that some kind of federal solution is desirable and necessary for the United Kingdom—and, by extension, for other European states as well.

The book contains useful historical sections that review the distinctive paths taken by each peripheral region in its incorporation into the United Kingdom. A helpful discussion of the controversy over proposals for devolution is included, along with a welcome chapter on English nationalism.

Political Integration has much to commend it at the descriptive level; it is less satisfactory at the analytical level. Analyses of peripheral nationalism pose three principal heuristic challenges: how to account for its social base, its strategies for political mobilization, and its timing. Little of substance is offered here from the materials in the British case—except for the interesting speculations about the timing of Celtic nationalism. It

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is all very well to argue that ethnic identities endure, but this cannot help explain the relative strength of Welsh as against, say, Cornish nationalism. It may be difficult to carry out research on the social bases of ethnic identity, but this important problem will not disappear merely by sweeping it under the nearest rug. Similarly, the book does not provide a single clue about how the nationalist parties go about attracting and mobilizing potential constituencies.

These lacunae will surely appear grievous to the theoretically minded reader. But to those who share the author's skepticism about the prospects for a systematic political science (see p. 34) there may be less cause for concern.

MICHAEL HECHTER
University of Washington

The Supreme Soviet: Politics and the Legislative Process in the Soviet Political System *by Peter Vanneman. Durham, N.C., Duke University Press, 1977. 256 pp. $11.95.*

Tests of the popular hypothesis that the Soviet system will adapt to modernism rather than collapse or radically change have tended to bypass the study of institutions, especially of the USSR Supreme Soviet. Small wonder at first glance. Does not this largest of world parliaments have the shortest and most preprogrammed sessions? Yes. Is it not a "rubber stamp" parliamentary facade? Not entirely, if one considers the intersessional roles of the Presidium of the Supreme Soviet (S.S.) and the recently burgeoning commission complex associated with that Party-controlled legislature, as Peter Vanneman shows in his book. It bridges an important gap in our understanding of just how the Party is going about adapting the unwieldly Soviet state bureaucracy.

Vanneman's spare, and sometimes formalistic, narrative begins, as it should, with a chapter on "The Russian and Soviet Institutional Heritage." Most of the subsequent chapters contain many materials that are either gathered in one place or published for the first time. This applies especially to Chapter 2, "The S.S. as the Constitutional Centerpiece of the Soviet Juridical Order"; Chapter 3, "Composition, Elections, and Legitimacy"; Chapter 4, "How the S.S. Works"; Chapter 6, "The Evolution, Organization, and Functioning of the Commission System of the S.S."; Chapter 8, "The Presidium of the S.S."; and Chapter 9, "The Communist Party and the S.S." These chapters contain the most solid materials of this sometimes unevenly substantiated, but generally valuable, analysis.

Not only is the Supreme Soviet turning out more laws and edicts than it did in its earlier, Stalinist years. Through its standing commissions (recently burgeoning to 26), the S.S. system is playing a growing role in the politics of drafting legislation (through wide consultation of agencies and specialists) and in "kontrol" (monitoring, not directing) below the Council of Ministers level over how the legislation is carried out—be it economic plans, the budget, or laws governing specific areas of Soviet life, from criminal codes to antipollution law.

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This institutional adaptation of the S.S., one may gather fom Vanneman's study, reflects several trends and facets of the gradual political adaptation to the "scientific and technical revolution" now being attempted by the Soviet regime. First, the soviets of workers' deputies (since 1977, soviets of people's deputies) have been enlivened from bottom to top since the late 1950s. Second, the Brezhnev coalition has pulled back from Khrushchev's attempted fragmentation and decentralization of the cumbersome and inefficient Soviet bureaucracy. The regime has decided to continue to rely on that bureaucracy but to try to make it work more efficiently by stepping up monitoring by specialists and the public through organizations like People's Control and the standing commissions of the soviets at local, Union Republic, and USSR levels. It is not a surge of democratization from below, but the Party's search for greater economic efficiency and social order that underlies the S.S.'s growing involvement in the Soviet policy process. Any shift of basic factional conflict over policy from the Party summit to the Supreme Soviet is only a potentiality, not an actuality. This does not gainsay a third tendency, just beginning to emerge, for opinion and interest groupings clustered around various issues to begin to seek to influence legislative outcomes in arenas of the S.S. like the standing commissions, the agenda-setting Council of Elders, and the S.S. Presidium. In this reviewer's long-standing opinion, forms do not take on a life of their own. But, as Vanneman shows, the Party has breathed life into some forms. It has thus set precedents of wider involvement in the policy process that will be difficult and operationally costly to reverse.

Vanneman's useful book prompts some questions for further exploration. Does the S.S. really legitimate Soviet rule in the ways depicted here? Is its ombudsman function significant? And on this point, what actually happens to petitioners, especially the more dissident petitioners, who turn to the S.S. Presidium office for redress of their grievances? Do Soviet people actually vote in the high percentages listed in Soviet election reports? What actually is the pattern of popular attitudes toward elections?

All these questions aside, the book under review makes a long overdue contribution to analysts and students of Soviet politics, provided that they come to the book already familiar with the broad outlines of Soviet history and Soviet rule.

PETER H. JUVILER
Barnard College, Columbia University

The Rise and Fall of the Cyprus Republic *by Kyriacos C. Markides. New Haven, Yale University Press, 1977. 200 pp. $12.50.*

The Cyprus crisis has been variously interpreted as a flawed colonial liberation struggle, as a clash of irreconcilable national and ethnic aspirations, as the result of external interference and intercommunal strife, or as a tragedy of mutual ignorance, misunderstanding, and political myopia. In this book Kyriacos C. Markides considers it from the perspective of political sociology. He has sought to trace "the internal social dynamics which, along with external interference, brought down the Cyprus Republic in the summer of 1974" (p. xiv). Although he considers it "axiomatic" that the "major respon-

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sibility for the destruction of Cyprus lies outside Cyprus" (p. xv), Markides has chosen to leave to others the international factors, except for that of external interference. Thus the title is something of a misnomer.

From the outset, Markides examines the ways in which a conventional anticolonial liberation movement was turned into a right-wing, church-led struggle for *enosis* (union with Greece) which in turn generated violent Turkish Cypriot and Turkish opposition and thus became, in his view, a case of unrealizable and even suicidal aspirations. After this illuminating beginning, the author's treatment of the Turkish Cypriot side of the story, while not hostile, becomes skimpy, since he tends to view those people from the outside, as an alien force rather than as an inherent part of the process. As a consequence, his accounts of the intercommunal confrontation (1963–74) and of the role of the United Nations in keeping the peace and contributing to normalization in the island are somewhat meager.

Markides's excellent chapter about Archbishop Makarios focuses on his shifting roles as traditional ethnarch, charismatic leader, president of a modern but divided republic, and ruler of a substantial economic empire. Notwithstanding his exceptional qualities, he "seems to have failed as a statesman," a judgment that the author bases on the rather opaque reason that he presided over the Cyprus Republic in a charismatic manner "instead of strengthening the legal-rational base of authority" (p. 55). This would seem to be one of those sociological explanations that raise more questions than they answer. In other sections of the book, however, the author gives a far clearer account of Makarios's mistakes of policy and inconsistencies of judgment that in 1974 paralyzed him politically and opened the door to the National Guard coup. In their struggle for *enosis*, his foes were fighting for the very cause he had sworn to uphold, and they used the methods, including terrorism, that Makarios had accepted during the EOKA revolt of 1955–1959.

What emerges clearly is the preposterous naiveté of certain more or less official theories that depicted the Archbishop as the "Castro of the Mediterranean" (p. 126). The fact is that while he enjoyed the consistent support of the left wing, Makarios never swerved from his conservative-liberal domestic policies and laissez-faire economics and never named a single socialist or communist to his cabinet. He did of course accept Third World, Arab, and Soviet backing on the international scene, since these forces supported Cypriot independence and opposed partition, which was not always the case in the West.

A useful chapter is devoted to the "disloyal opposition." Markides perceptively gauges the limited impact of pro-*enosis* sentiment upon various groups of the Greek Cypriot population. During the late 1960s and early 1970s, the fear of Turkish intervention combined with economic prosperity and general dislike of the right-wing dictatorship in Greece to reduce active support of *enosis* to small but violent groups of malcontents. These would have remained an insignificant minority, but for the support of the Athens junta that was determined to overthrow the Cyprus Republic.

The tragic course pursued by the "disloyal opposition" and its foreign backers is ably traced in the last chapter. Markides finds that two "unsolvable problems" precipitated the breakdown of democratic government: the intercommunal question and the related issue of *enosis*. Despite the efforts of United Nations representatives, the intercommunal talks, according to Markides, had no chance of succeeding. The preconditions for accommodation, such as a shared commitment to maintenance of the existing order and accept-

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ance of a reasonable balance of power between the ethnic groups, were lacking. External threats did not unite the two communities but pulled them apart, since these threats were invariably directed against one of them for the benefit of the other. What sealed the doom of efforts at accommodation was, again, external interference.

Markides's epilogue offers a brief but unsatisfactory account of United States policy toward Cyprus, with special reference to Mr. Kissinger's admitted mistakes. Also unconvincing are Markides's concluding recommendations, largely based on Robert Stephens's book, *Cyprus: A Place of Arms*, which proposed that the island be made into a permanent training ground for United Nations troops.

Despite its dispensible sociological apparatus, Markides's book provides a well-written and often fascinating account of major aspects of the history of the Cyprus Republic, especially the political disintegration of the Greek Cypriot community, fomented by agents of the Greek military regime. Within his intentionally limited scope, the author has done a very good job. His book will no doubt remain the standard account of Greek Cypriot sociopolitical developments culminating in the tragedy of 1974.

GEORGE L. SHERRY
*The United Nations**

* The opinions expressed in this review are those of the reviewer and do not necessarily reflect the views of the Secretariat of the United Nations.

Revolution and the Transformation of Societies *by S.N. Eisenstadt. New York, The Free Press, 1978. 348 pp. $15.95.*

This ambitious and wide-ranging book by the dean of Israeli sociology sees as its central task "to analyze under what conditions and in what historical circumstances . . . revolutions and revolutionary transformations occur" (p. xv). The author attempts to paint a vast canvas covering primitive societies, the patrimonial regimes of, for example, Assyria and Babylon, India, and ancient Egypt, the imperial systems of, for example, Rome, China, and Russia, as well as city states and tribal federations. Only after this huge territory has been surveyed, and different sources of transformation and change have been identified, does Eisenstadt turn to an explanation of the "true revolutions" of modern history.

Rejecting previous approaches to the subject of revolutions, which tended either to concentrate attention on social psychological variables or to focus exclusively on class conflicts, the author proposes his own complex explanatory scheme. While not neglecting the contention between classes, he pays analytical attention to, among others, the relations between the center of societies and the periphery, the interweaving of elites with broader social movements, the political articulation of feelings of relative deprivation by political entrepreneurs, the mobilization of resources by status quo forces as well as by revolutionary contenders and, perhaps most important, the symbolic orientations and codes which prevail within societies and foster or hinder revolutionary challenges.

While much of Eisenstadt's earlier work stood under the shadow of the evolutionary perspective elaborated by, *inter alia*, Parsons and Bellah, he now argues that "at seem-

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ingly similar levels of technological development or structural differentiation we find . . . social structures very diverse in form: The dimensions of differentiation are not equally elaborated in them" (p. 65). Moving closer to a conflict perspective, he now argues that "in any social order considerable dissension over the distribution of power and scarce resources" is to be expected (p. 41). Granted, he argues, that ground rules of social interaction rooted in complicated symbolic codes tend to cement these societies, "the very construction and institutionalization of the ground rules . . . creates the possibility of tensions, conflicts, and contradictions leading to change" (p. 40).

It is impossible to do justice to Eisenstadt's complex argument in a short review. I can only indicate here that his major claim is that revolutionary transformations, as distinct from revolts or other forms of internal violence, tend to occur only in imperial societies, more particularly in those that have strong built-in tensions between transcendental and mundane orientations. In patrimonial societies, as well as in the other social formations discussed, social change does not take revolutionary forms even though violent rebellions and protests may mark its course. Rebellion, conflict, and change, Eisenstadt concludes, are to be found in all types of societies. "True revolutions," in contrast, amounting to rare mutations in societal conditions, could occur only under very special structural, symbolic, and organizational circumstances in the last few hundred years. They are not likely to re-emerge in the future.

Eisenstadt's analysis moves on a highly abstract and generalized plane. His exposition is written, as it were, in shorthand. He hardly ever bothers to flesh out the bare bones of his schematic theoretical scheme with specific illustrations. The exposition is complex, badly organized, often repetitious, and hard to follow. The intricate and elaborate theoretical construction too often leaves the reader wondering whether the author, juggling with so many variables and tendencies, has provided a workable scheme that others might be able to apply concretely in their research, or whether he has only provided an elaborate Byzantine system of categories devoid of explanatory power.

The detailed multilingual bibliographies appended to each chapter are extremely helpful and are in themselves worth the price of admission.

LEWIS A. COSER
State University of New York at Stony Brook

China's Future: Foreign Policy and Economic Development in the Post-Mao Era *by Allen S. Whiting and Robert F. Dernberger. New York, 1980s, Project Council on Foreign Relations, 1977. 202 pp. Cloth, $8.95; paper, $5.95.*

These essays were written in 1976 for the 1980s Project of the Council on Foreign Relations, by well-known academic specialists on Chinese foreign policy and the Chinese economy. They were written at a time when China was locked in bitter political conflict over the succession to Mao Tse-tung and the relative priority to be given to economic development and revolutionary change. The essays were completed just after Mao's death and the momentous purge of China's radical leaders. China is about to enter the

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third year of the post-Mao era; how well have the authors done in forecasting the evolution of China's foreign and economic policies?

Actually, the book is concerned only in part with projections into the future. Much of it is taken up with a review of past performance, a useful exercise which serves to remind the reader of the many twists and turns in China's policies. Sketching past changes permits the authors to warn the reader that since the unexpected occurred frequently enough in the past, it may again occur in the future. Reviewing the past also permits the authors to identify continuities and constants in Chinese policies, which are likely to continue to limit the range and duration of future changes.

Whiting presents a nuanced and informed survey of the major problems facing China's foreign policymakers. He offers cautiously optimistic assessments of the Taiwan problem—"compulsive irredentism" not being a characteristic of PRC behavior—and discusses also such issues as the Chinese approach to nuclear proliferation and conventional arms transfers. Whiting forecasts growing tension between China's orientation toward the industrialized "north" and the underdeveloped "south," predicting that a "gradual and subtle" shift will occur away from identification with the Third World and toward growing interdependence with the industrial world. His central point is, in fact, that China will gradually become more integrated into the present international system.

Dernberger also presents a cautiously optimistic assessment of China's economic prospects. The speed of development is likely to decline somewhat, to a still highly respectable 5 percent rate in growth of GNP between 1975 and 1990. In industry, somewhat lower growth rates will result from the fact that China has already taken advantage of most of the gains of "extensive development" and will now have to make more expensive investments with lower rates of return. But "the single most important determinant of China's economic future over the next 15 years will be the ability to increase agricultural production more than 2 percent annually" (p. 126). Dernberger, after a careful review of the basically unfavorable conditions for increasing the agricultural growth rate, concludes that China is not likely to be able to grow more rapidly than 2 percent. China will develop, but not at a spectacular pace. Dernberger sees radical influences as obstacles to rational economic decision making, but feels that economic necessity is bound to incline China onto a moderate course.

With a few exceptions, the authors' well-reasoned and well-informed prognoses have thus far been borne out. Yet, what is missing from their forecast is a sense of the intensity of the mood and policy swing that has occurred in Peking since Mao's death, as the new leaders have embraced the goal of all-out modernization. China's leaders have adopted extremely ambitious targets—agriculture is to grow at 4–5 percent annually—and they have turned to the outside world in a far more dramatic way than was thought possible a few years ago, concluding long-term trade agreements and deciding to send thousands of students abroad. Perhaps the current program of all-out modernization will again be disrupted by a nativist-radical attack, but for the time being one cannot but marvel at the decisiveness with which many radical shibboleths have been cast aside.

Despite hints at the possibility of drastic policy change, the authors did not forecast the dramatic turn in events. Could they have done so from the vantage point of 1976? Perhaps a careful examination of the policy debates of 1975–76 might have alerted them

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to the depths of the underlying dissatisfaction with the Cultural Revolution. Yet, I doubt it. The fundamental lesson offered by this study, then, is that forecasting the future is not a very fruitful enterprise when key variables are only poorly understood.

THOMAS P. BERNSTEIN
Columbia University

The Foreign Policy of Modern Japan *edited by Robert A. Scalapino. Berkeley, University of California Press, 1977. 426 pp. Cloth, $21.50; paper, $5.45.*

The title of this volume of essays by thirteen contributors is slightly misleading. Few of the thirteen pieces examine "the foreign policy of modern Japan," and those that do go over mostly familiar territory, giving a summary of security, trade, and other concerns of Japanese policy since the nineteenth century. Rather, the bulk of the essays deals with recent, that is, postwar through 1974 developments. Because they were originally written for a conference held in January 1974, they inevitably stress themes that appeared important to the participants at that time. There is always a risk in publishing conference papers, since national moods and concerns change constantly. This is probably why similar volumes keep cropping up.

This volume is distinguished, however, by its primary emphasis on the institutional contexts of Japanese decision making which have remained more or less stable since the war. Because Japanese foreign policy—like that of any other country—too often tends to be viewed, at one level, in terms of the overall mechanisms of international politics and, at another, of the nation's cultural peculiarities, it is good to be reminded of the specific contexts in which cultural assumptions and aspirations are translated into the language of power. It is the political parties, bureaucracies, and business organizations that perform this function, and so it is no accident that the best essays in this book are analyses of these institutions. Hans Baerwald demonstrates convincingly that Diet deliberations have been less important than discussions within the Liberal Democratic Party in formulating policy. Haruhiko Fukui makes a detailed statistical study of the Foreign Ministry, as Chalmers Johnson does of the Ministry of International Trade and Industry. They describe the two agencies' internal organization, systems of personnel recruitment, legal foundations of their jurisdiction, and the ways in which decisions are reached within the ministries. Johnson's essay, in particular, should be required reading for all those who would separate myth from reality about the "omnipotent" MITI. He shows that MITI's powers are in fact severely curtailed by law and custom, and that even its authority over international economic matters is not total, other ministries jealously guarding their own spheres of influence.

The essays by Gerald L. Curtis and Sadako Ogata complement the above three studies by analyzing two episodes (the Tyumen oil development project and the normalization of diplomatic relations with the People's Republic of China) in which government and business closely interacted. Curtis argues that the Japanese government left the initiative for the Tyumen project to the business community, specifically those firms that were represented in the Japan-Soviet Economic Committee, but that there was close consulta-

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tion and coordination between the two at all stages of the discussion. Ogata shows that a pro-Peking shift in some top business leaders had a considerable impact on Japanese policy. These decisions, of course, were not taken in a vacuum. As Akio Watanabe reveals in his essay on Japanese public opinion, business and political leaders were acutely aware of public moods, but these were usually reactive to decisions made, not generative of new initiatives.

Whether these characteristics of Japanese decision making will undergo change is a question these and other authors in this book address themselves to. Japanese institutions, as Masataka Kosaka notes, on the whole served the nation well after the war, but the question is whether, in their preoccupation with harmonizing various interests and concerns of the people, they did not tend to generate nationalistic orientations and parochial outlooks. In today's world, they will more and more have to consider not simply domestic but also international harmony and stability. This book provides a good and factual point of departure for raising that crucial question.

AKIRA IRIYE
University of Chicago

Policy Making in Contemporary Japan *edited by T. J. Pempel. Ithaca, N. Y., Cornell University Press, 1977. 345 pp. $17.50.*

Over at least the past decade, American scholars have been fascinated with the Japanese decision-making process. The book under review, nine chapters by six authors, one of whom served as editor, constitutes a further contribution to this literature.

Four authors present case studies. Haruhiko Fukui explains how Japan went about recognizing the People's Republic of China. John Campbell describes how Japanese repatriates maneuvered to secure compensation from the Japanese government for property seized as a result of World War II. Michael Donnelly explains how the Japanese farmers dicker to get the government to offer a high price for their rice. Margaret McKean describes how the government, the political parties, business, and labor came to terms with the growing public concern with pollution.

Other chapters contain a broader perspective. In his second chapter, Haruhiko Fukui reviews the literature on Japanese decision making. He concludes that the thesis that Japan is controlled by a triumvirate of conservative politicians, big businessmen, and senior bureaucrats is insufficient. He calls for a pluralistic thesis which will include citizens' movements, the opposition parties, and local political leaders. Using 1968 as his test year, Bradley Richardson examines seven routine policy-making scenarios and draws from them a conceptual scheme for organizing and interpreting the policy-making behavior of political actors. Using higher education as an example, T. J. Pempel points out that public policy has been formulated in vastly different ways. He isolates three patterns of particular interest: policy making through group conflict; pressure-group pluralism; and incremental policy making.

T. J. Pempel also wrote the introduction, the conclusions, and served as editor. He was the organizer and the chairman of the panel at the Association of Asian Studies when

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most of this book's chapters first surfaced as papers. The book owes its parturition to him more than any other contributor.

In 1975 Pempel reviewed another book on Japanese organization and decision making. [The book was Ezra Vogel, ed., *Japanese Organization and Decision-making*, University of California Press, Berkeley, 1975. The review appeared in the *Japan Interpreter* (Summer 1975) pp. 238–242.] In this review he identified three needs. First, he called for studies with less abstraction and more empirical investigation. Only three of Pempel's authors harkened to this call. Bradley Richardson tried to marry "empirical theory and case studies" (p. 239). Second, Pempel called for further investigation into how the structure of decision making affects the decision itself. None of Pempel's authors tangled with that question. Third, Pempel asked for more international comparisons, preferably comparisons with nations other than the United States, since "behavior in the United States may be almost as atypical as the Japanese behavior for which it serves as the standard of measurement" (p. 241). None of Pempel's authors explicitly attempted a cross-national analysis but four of Pempel's authors, including Pempel himself, built analytical models from or otherwise used the theory of Theodore Lowi, who developed his ideas from American case studies, particularly a study of American foreign trade.

In his 1975 review Pempel concluded that "Japanese behavior appears exceedingly diverse and not amenable to over-generalization" (p. 240). In the book under review, Pempel reiterates his conclusion, "there is tremendous diversity in Japanese policy-making defying most generalizations" (p. 310). I support Pempel's conclusion. But I find in it an indictment of his book. While he has brought together some more case studies and encouraged several authors to develop some new ways of typifying them, he has not greatly advanced our knowledge of Japanese policy making.

NATHANIEL B. THAYER
The Johns Hopkins University

Repression and Repressive Violence *edited by Marjo Hoefnagels. Amsterdam and Lisse, Swets & Zeitlinger, 1977. 194 pp. $17.00.*

This slim volume brings together 11 papers presented at an international conference on "Repression and Repressive Violence" held at the Free University of Brussels in November 1976.

The book's overall value lies in its direct focus on governmental violence in democratic and nondemocratic societies. 'The objective of the conference,'' notes Marjo Hoefnagels, "had been to create awareness of the fact that repression and repressive violence are not merely phenomena which take place in distant countries, under clearly authoritarian regimes, but can and do happen in 'democratic' societies as well, albeit in a sophisticated and more elusive way" (p. vi).

The 15 contributors to the volume are European scholars, five of whom have been educated in the United States. The papers vary widely in length, subject matter, style, clarity, and sophistication. The opening paper—Beverly Woodward, "Moral Reasoning and Repressive Violence"—is alternatively sensible, elusive, and pretentious. Johan

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Niezing's "Repression and Democratic Theory: Some Skeptical Remarks" is opaque and runs a mere five pages. Other contributions focus on such topics as "Political Violence and Peace Research," "Repressive Violence: A Legal Perspective," "In Memoriam: Constitutional Rights and the Liberal State," and "Psychological Genocide."

Steve Wright's "An Assessment of the New Technologies of Repression"—the longest essay in the book (32 pages)—is literally awesome in detailing such subjects as "area denial technology," "communications technology," "computers," "dispersive and coercive technologies," "intruder detection technology," and "surveillance technology." The paper includes a four-page appendix listing names and addresses of "a sample of the [American and European] companies involved in manufacturing and supplying government agencies with technologies for use in the process of social control" (p. 161).

While the diversity of contributions is understandable in any endeavor of this sort, the editor of *Repression and Repressive Violence* clearly missed the opportunity to give the volume some semblance of cohesion by adding a substantive introduction. His one-page preface is inadequate to address the many vital issues raised in the book.

MOSTAFA REJAI
Miami University, Ohio

The Politics of Solzhenitsyn *by Stephen Carter. New York, Holmes & Meier Publishers, 1977. 162 pp. $12.50.*

Among the émigrés from post-Stalin Russia, Alexander Solzhenitsyn is not only the most famous writer, but also the most important political figure. In company with so many Russian novelists past and present (Gogol, Dostoyevsky, and Tolstoy, to mention but three) Solzhenitsyn is not content to be a "mere" artist but regards it as his duty to offer a reasoned critique of his country's political *status quo*. Like the others, he also offers a blueprint for a better Russian society. Unlike his predecessors, however, Solzhenitsyn feels that it is incumbent upon him to compile a historical record of his country's travail during the past sixty years. His forerunners did not have to do that. However oppressive the tsarist regime may have been at times, it never made a concerted and systematic effort to suppress certain pages of Russian history.

All of this makes Solzhenitsyn an eminently political novelist. It is therefore fitting that his views should be examined in a separate study by Stephen Carter, a senior lecturer in politics and government at the City of London Polytechnic. If Solzhenitsyn's political opinions were to be deduced solely from his public pronouncements and published nonfiction, there would be no need to quarrel with any attempt at distilling from these a more or less consistent ideology. In Solzhenitsyn's case, however, one clearly cannot omit from consideration his highly politicized fiction. On the other hand, doing so results in a methodological difficulty of which Stephen Carter is well aware:

> I realise that there is a problem of attribution in much of the literary analysis. For example, are the ideas expressed by Varsonifiev in *August 1914* those of Solzhenitsyn? Does Kostoglotov in *Cancer Ward* exactly mirror the author's views at that time? And what are we to say of such characters as Shulubin in *Cancer Ward*, who advocates a philosophy of 'ethical socialism'? I

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have tried to solve this problem by attempting to locate themes which are in some sense pervasive; and I trust that the reader will allow me to rely on occasions on plausibility and the application of commonsense (p. xii).

Carter's book is organized much as a textbook would be. Thus, individual chapters are devoted to such problems as Soviet courts, Soviet penology, and Soviet concentration camps. Literary and nonliterary materials are carefully analyzed and the reader is then offered a convenient summary. Thus the section on society, reality, and the individual ends as follows:

In this chapter, I have shown that Solzhenitsyn depicts the Soviet experience as responsible for: suppression of individual rights and freedom of thought (including the degradation of the Russian language); creation of mutual mistrust and paranoia; attempted distortion of the public's perception of reality and history; creation of an atmosphere conducive to widespread cruelty and indifference in people's mutual relations, the social services, and among the young; economic inefficiency; the promotion of criminals and the suppression of the innocent and the talented; and finally the paradoxically beneficial effects of the camps on some individuals which may enable the truth eventually to be told (p. 55).

There are also chapters dealing with specific works of Solzhenitsyn. Thus, in commenting on *Lenin in Zürich* and alleged similarities of Lenin and Solzhenitsyn, Carter writes:

Solzhenitsyn in Zürich, like Lenin before him, has been a man totally dedicated to revolution; but whereas Lenin was concerned with a political revolution, Solzhenitsyn hopes to achieve a moral revolution. He believes that all governments must be based upon an ethical idea, and that democracies tend to lack this, so that they often degenerate into unedifying struggles between rival interests. He believes that authoritarian governments are preferable to unstable democracies, because it is the latter which give rise to totalitarianism. He gives no evidence for this important assertion, apparently deriving most of his conclusions from Russian experience. Because Solzhenitsyn regards all communism as evil, he feels that it should be resisted wherever possible, and on these grounds he opposes the policy of détente as practised at present by the West and points to the declining influence of the West in world affairs and her inadequate defences. His aim, however, is not to foment war but to minimise its possibility and at the same time check the spread of communism. Solzhenitsyn has clarified his differences with other Russian dissidents such as Pavel Litvinov, Andrei Sakharov and Roy Medvedev, but he seems to be something of a distant, lonely figure to his fellow countrymen, although admired greatly by them. In this respect the comparison with Lenin remains an intriguing one (pp. 138–39).

This is an important and timely short book that should be of interest to anyone concerned with the Soviet Union, or for that matter, with contemporary politics in general.

MAURICE FRIEDBERG
University of Illinois at Urbana—Champaign

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Reference Books and Source Materials Received

Annual Review of Sociology, Vol. 4 *edited by Ralph H. Turner, James Coleman, and Renée C. Fox. Palo Alto, Calif., Annual Reviews Inc., 1978. 632 pp. $17.00.*

Criminal Victimization in the United States: A Comparison of 1975 and 1976 Findings *prepared by the Bureau of the Census. Rockville, Md., National Criminal Justice Reference Service, 1977. 76 pp. Single copy at no charge.*

Directory of Federal Statistics for Local Areas: A Guide to Sources, 1976 *prepared by the Data User Services Division, Bureau of the Census. Washington, D. C., U.S. Government Printing Office, 1978. 363 pp. $5.50.*

Encyclopedia of American Foreign Policy, Vols. 1-3: Studies of the Principal Movements and Ideas *edited by Alexander DeConde. New York, Charles Scribner's Sons, 1978. 1201 pp. $99.00.*

Expenditure and Employment Data for the Criminal Justice System 1976 *prepared by the Law Enforcement Assistance Administration and the Bureau of the Census. Rockville, Md., National Criminal Justice Reference Service, 1978. 400 pp. Single copy at no charge.*

International Relations Theory: A Bibliography *edited by A. J. R. Groom and C. R. Mitchell. London, Frances Pinter Ltd. and New York, Nichols Publishing Co., 1978. 222 pp. $20.00.*

Labor-Management Relations in State and Local Governments: 1976 *prepared by the Department of Commerce and the Department of Labor. Washington, D.C., U.S. Government Printing Office, 1978. 166 pp. $3.25.*

1977 Census of Governments, vol. 1, Governmental Organization *prepared by the Bureau of the Census. Washington, D. C., U. S. Government Printing Office, 1978. 516 pp. $6.75.*

The Police and Public Opinion: An Analysis of Victimization and Attitude Data from 13 American Cities *prepared by the Law Enforcement Assistance Administration. Rockville, Md., National Criminal Justice Reference Service, 1977. 63 pp. Single copy at no charge.*

Political Handbook of the World: 1978 *edited by Arthur S. Banks. New York, McGraw-Hill Book Company, 1978. 627 pp. $24.95.*

Trends in Expenditure and Employment Data for the Criminal Justice System 1971–1976 *prepared by the Law Enforcement Assistance Administration and the Bureau of the Census. Rockville, Md., National Criminal Justice Reference Service, 1978. 150 pp. Single copy at no charge.*

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United States Congressional Districts 1788–1841 *edited by Stanley B. Parsons, William W. Beach, and Dan Herman. Westport, Conn., Greenwood Press, 1978. 416 pp. $35.00.*

U. S. Defense Policy: Weapons, Strategy and Commitments *edited by Margaret Thompson. Washington, D. C., Congressional Quarterly Inc., 1978. 202 pp. $5.95.*

Washington Information Directory 1978–79 *edited by Robert E. Healy. Washington, D. C., Congressional Quarterly Inc., 1978. 902 pp. $19.50.*

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Correspondence

To the Editor:

Had David Fellman's review (*PSQ*, Spring 1978) of my "Government by Judiciary: The Transformation of the Fourteenth Amendment" not gained respectability from publication in the *Quarterly*, I would not feel compelled to respond. Putting to one side slurs such as "a very curious remark for an educated man," Fellman's review is replete with reckless and slippery misstatements: Berger "devotes an enormous amount of space to . . . the racial segregation decision" when in fact only 16 out of 450 pages are devoted thereto. Another distortion: "Berger is certain that the only thing that the 39th Congress had in mind [by adoption of the 14th Amendment] was to constitutionalize the Civil Rights Act," as if this were idiosyncratic, when in fact there was no dissent. The facts are even stronger. Charles Fairman summarized the history: "The provisions of the one are treated as though they were essentially *identical* with those of the other." In the words of one of the framers, George Latham, the Act "covers *exactly* the same ground as the amendment."

Fellman cannot "determine with Berger's absolute certainty" just what the framers "had in mind"; apparently he rejects the conviction that they "did not intend to create federally enforceable limitations" respecting "racial segregation and voting." But another critic, Professor Nathaniel Nathanson, observed that Alexander Bickel "quite conclusively demonstrated" that the Amendment "would not require school desegregation"; that Justice Harlan "convincingly demonstrated" the same regarding suffrage; and that "Berger's independent research and analysis confirms and adds weight to those conclusions." In the *London Times*, Professor Max Beloff, formerly of Oxford and now head of one of the "red brick" universities, referred to my "incontrovertible conclusion" to that effect. Professor Gerald Gunther wrote that "most constitutional lawyers agree" that the "one person-one vote" doctrine "lacks all historical justification." Fellman resolutely shuts his eyes to the facts.

Again, "according to Berger, the only way we can depart from the original intent is to amend the Constitution through the procedure spelled out in Article V." Not merely "according to Berger"; Hamilton declared that "Until the people by solemn and authori-

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tative act, annulled or changed the established form, it is binding . . . and no presumption, or even knowledge of their sentiments, can warrant their representatives in a departure from it prior to such an act." As Justice Black often said: "The Constitution makers knew the need for change and provided for it" by the amendment process of Article V. The blank check Fellman would give the Court has yet to be signed by the American people.

Scarcely a line of Fellman's review is not equally vulnerable, but space confines me to a last remark, that of a cool-eyed Briton, Professor Beloff: "The quite extraordinary contortions that have gone into proving the contrary [of my study] make sad reading for those impressed by the high quality of American legal-historical scholarship."

RAOUL BERGER
Concord, Mass.

Mr. Fellman replies:

I appreciate the invitation to respond to Raoul Berger's response to my review of his book *Government by Judiciary*. It has always been my practice not to get into debates with authors of books that I have reviewed, and I do not care to do so in this instance. I would prefer to let my review stand as an expression of my views. Of course I do not derive pleasure from the fact that my review made Berger so unhappy.

DAVID FELLMAN
University of Wisconsin-Madison

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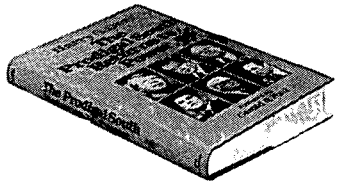

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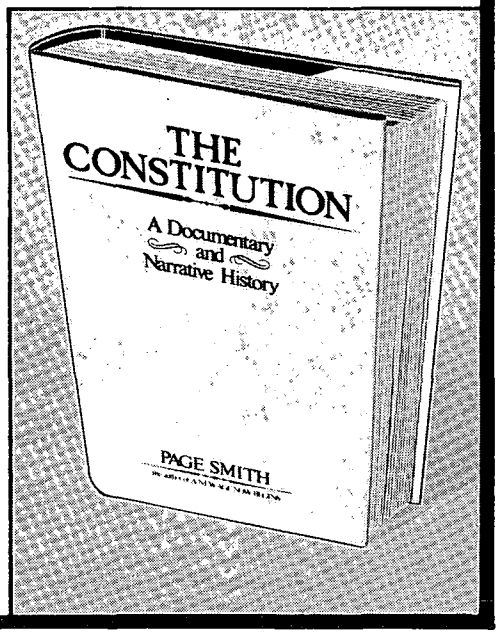

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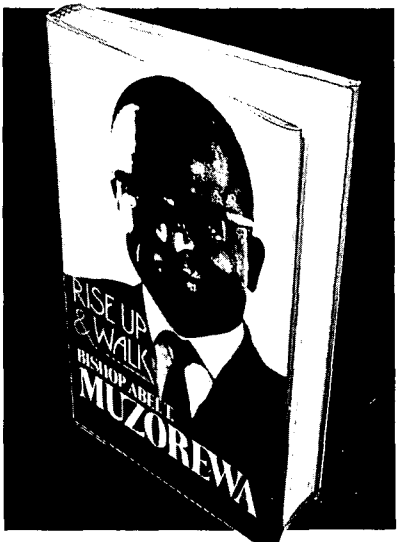

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